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VOLUME VII.

SESSION 1889-1890.

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PAPERS READ BEFORE THE SOCIETY.

SESSION 1889-1890.

PAGE

I.—ARCHÆOLOGY IN BRITAIN, 1877-1889 1

By JAMES KENWARD, F.S.A.

II.—ON THE EFFECTS OF A REPUTED WATERSPOUT AT BATCOMBE AND CHETNOLE, DORSETSHIRE 26

By REV. G. DEANE, D.Sc., B.A., F.G.S.

III.—EXAMPLES OF "SOLUTION-COMPOUNDS" 33

By G. GORE, LL.D., F.R.S.

IV.—RELATIVE AMOUNTS OF AVAILABLE VOLTAIC ENERGY OF AQUEOUS SOLUTIONS 43

By G. GORE, LL.D., F.R.S.

V.—THE GLACIATION OF GREENLAND.. 63

By WM. SHERWOOD.

VI.—ON THE STUDY OF EARTHQUAKES IN GREAT BRITAIN .. 68

By CHARLES DAVISON, M.A.

VII.—THE BOULDERS OF THE MIDLAND DISTRICT 85

By FRED W. MARTIN, F.G.S.

VIII.—THE BARR BEACON BEDS.. 113

By JOSEPH LANDON, F.G.S.

IX.—NOTE ON A NEW DRIFT SECTION AT NORTHFIELD 127

By JOSEPH LANDON, F.G.S.

X.—BIMETALLISM: HOW ITS BENEFITS (AND OTHER PUBLIC ADVANTAGES) MAY BE ATTAINED AND ITS EVILS AVOIDED.. 132

By ALFRED HILL.

XI.—THE STRUCTURE AND FUNCTION OF THE AIR-BLADDER IN CERTAIN FISHES 144

By Prof. T. W. BRIDGE, M.A.

SESSION 1890-1891.

PAGE

I.—THE FUTURE OF GEOLOGY 189

By the Rev. G. DEANE, D.Sc., B.A., F.G.S.
(President of the Society.)

II.—A GRAPHICAL METHOD OF EXPLAINING THE DIFFRACTION BANDS AT THE EDGE OF A SHADOW 210

By J. H. POYNTING, F.R.S.
(Professor of Physics, Mason College, Birmingham.)

III.—INVESTIGATIONS IN ARTIFICIAL TERATOGENY 220

By BERTRAM C. A. WINDLE, M.A., M.D.
(Professor of Anatomy in the Queen's College, Birmingham.)

IV.—A NOTE ON "FIXED" AND "OCCULTING" OPTICAL APPARATUS FOR LIGHTHOUSES 233

By JAMES KENWARD, F.S.A., Assoc. Inst. C.E.

V.—THE EVOLUTION OF THE EARTH'S CRUST 241

By CHARLES CALLAWAY, D.Sc., M.A., F.G.S.

VI.—SOME RELATIONS OF ELECTROMOTIVE FORCE TO ATOMIC WEIGHT, CHEMICAL HEAT, ETC. 253

By DR. G. GORE, F.R.S.

VII.—MODERN DEVELOPMENTS OF THE ATOMIC THEORY 264

By WILLIAM A. TILDEN, D.Sc., F.R.S.
(Professor of Chemistry and Metallurgy in the Mason Science College, Birmingham.)

VIII.—THE CAUSE OF IMPERFECTIONS IN THE COLOUR OF SHEET BRASS 282

By THOMAS TURNER, Assoc. R.S.M., F.C.S., F.I.C.
(Lecturer on Metallurgy at Mason College).

IX.—OBSERVATIONS ON THE LAW OF FACILITY OF ERRORS 289

By the Rev. H. W. WATSON, D.Sc., F.R.S.

MAP.

PAGE.

THE BOULDERS OF THE MIDLAND DISTRICT 85

LIST OF PLATES.

NO. PAGE.

I., II., III., IV.—THE BARR BEACON BEDS 113

PROCEEDINGS
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BIRMINGHAM PHILOSOPHICAL SOCIETY.

VOL. VII.—Part I.

FOURTEENTH SESSION, 1889-90.

I.—Archæology in Britain, 1877-1889.

By James Kenward, F.S.A.,
President of the Society.

[Presidential Address, October 9th, 1889]

In 1877 I had the honour to read to the Society a paper entitled "The Place of Archæology in Science," in which I sought to review very succinctly the progress of archæology to that date, to acknowledge the scientific methods by which modern investigations in it had been conducted, and to show the relevance of it to our actual life and manners. I seek now to pursue the same course with regard to the past ten or twelve years, and with regard to Britain alone, meaning by those words the study of archaic subjects and objects relating to, or discovered in, our own country. The theme is a wide one, abounding in details,

and my address is in no sense an exhaustive record of opinions or facts, but only as much as I may give in an hour's space of what I have been able to collect from reading or observation in the brief intervals of professional work.

The most striking fact to be encountered *in limine* is, I think, the increased and increasing application of scientific training to antiquarian research. There is no direct connection between physical science and archæology—no educational machinery for popularising archaic studies—very little of State subsidy, or private endowment. Yet the growth of precision of thought, of sobriety of judgment, and of thoroughness of inspection which distinguishes our age, and especially the decade 1880-1890, has benefited these studies quite as fully as those of more "practical" import. The Archbishop of York, presiding at the anniversary meeting of the Palestine Exploration Fund in 1886, used these words: "We pledged ourselves that our scheme of work should be carried out on purely scientific principles—that is to say, we should not merely send out travellers, who should describe to us scenes and places which have been visited and described many times before, but we should submit all observations to critical scientific tests, and record them as a contribution to the science of the subject." How admirably the work in Palestine has been done, and the results presented to the world, is very widely known, and the propriety of Dr. Thomson's declaration is manifest.

Seven or eight years ago that fairest gift of science, the electric light, had no part in illustrating the treasures of the greatest shrine of archæology and literature in the world. *Now* not only is the reading room of the British Museum adequately equipped with it, but soon all the splendid collections there will be studied in the evening by its aid. Again, I well remember the aspect of the rooms of the Society of Antiquaries of London when we pored over old texts and relics by the doubtful help of oil lamps. *Now* we have the cool soft electric glow under which Dr. Schliemann may discourse comfortably of Troy and of Mycenæ, or Dr. John Evans of the Stone and Bronze Ages,

while no weariness steals over the eyes, and no headache flushes the brow. The electric light should, indeed, replace the time-honoured Roman lamps on the seal of the Antiquaries' charter, and the proud motto "Non extinguetur" will be doubly significant.

Does not this physical light of Science express exactly the intellectual light which she has focused on all modern studies, and notably on these I am submitting to you? By its aid—the aid of reason, of method, of scholarship,—the fields of antiquarian research have yielded far richer results than could be won by the work of untrained virtuososhiip or fanciful speculation.

Let us glance first at some traces or evidences of Primeval Man.

No very important addition has been made in this country to the small list familiar to geologists of human bones from the newer tertiaries or quaternary strata, but the recent indications of man are full of interest. The caverns of Cae Gwyn and Ffynnon Beuno, in the Vale of Clwyd, have been re-examined by a special committee of the British Association. As usual, a large quantity of bones of the hyæna, bear, mammoth, rhinoceros, horse, and other animals were found, and with them a variety of flint implements—flakes, scrapers, and lance-heads. After a prolonged inspection of the caverns, the committee decided that they were frequented by these pleistocene creatures and by man "before the characteristic glacial deposits of this area were accumulated." Dr. Henry Hicks and Mr. E. B. Luxmoore had previously examined and reported on the caves, asserting that man must have occupied them before the glacial drifts. There was, indeed, a hidden entrance at Cae Gwyn, blocked up by a thick bed of glacial deposits. A well-worked flint flake was found under these deposits. "It seems clear," says Dr. Hicks, "that the contents of the caverns must have been washed out by marine action during the great submergence in the glacial period, and then covered by marine sand and an upper boulder clay." He believes that all the flint tools were the work of preglacial man, and that preglacial man reached

this country from the north with the northern animals which supplied him with food.*

Previously to these explorations a very interesting limestone cavern at Llandudno had been opened out, in which were bones of the cave bear and other animals, as also a few human bones and some flint implements. Among the bones was the lower jaw of a horse with a rude ornamentation incised on it. I may add that Mr. Pengelly discovered near the head of the tidal estuary of the River Teign a bed of fine sandy mud full of the shells of *Scrobicularia piperata*, in which, at a depth of ten feet, were a number of human bones, all of the age of the deposition of the bed. He also found in Bench Cavern, Brixham, a single flint flake tool among abundant bones of the hyæna.

At Tor Bryan, near Newton Abbot, a large number of worked flints and animal remains have been extracted from the caves.

These, and other evidences from cavern and river deposits of the antiquity of primeval man, are vouched for by geologists of high repute, and admit of no doubt.

Next in order are the evidences of man of the so-called Neolithic, Bronze, and Iron Ages, which have been numerous in the decade I am considering. I cite only a few cases.

Professor Boyd Dawkins has described his researches in the Gop Cave near St. Asaph, and pointed out the oval-headed Iberic type of the skulls there, that is, the dolichocephalic shape belonging to the dark-featured, small-statured people which preceded in these islands the taller round-headed Gauls or Kelts. This early people of the Stone Age had skulls whose

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* In 1886, in a cave at Spy, province of Namur, in quaternary deposits, M. Freipont discovered two human skeletons, whose skulls more nearly resembled those of the anthropoid apes than any before known. From other indications, he concluded that the individuals must have walked with legs slightly bent. Further evidence is desirable, for other skulls of the same or greater antiquity, such as that from the Engis Cave, also in Belgium, are of pure Caucasian type.

breadth was to the length as 71 to 100, while the later people —the Kelts—left skulls with a relation of breadth to length as 81 to 100. It is noticeable that the dolichocephalic or long, or oval skull is found for the most part in the long barrow, while the brachycephalic or round skull is found generally in the round barrow.

General Pitt-Rivers, at Cranborne Chace, on the border of Dorset and Wilts, has discovered four British or Romano-British villages, with skulls of the oval type, and at Rotherley a Romano-British villa of exceeding interest. Of these and the other discoveries made by him on Cranborne Chace, he has given a minute account in two fine quarto volumes that will rank among the best standard books of Anthropology and Archæology. The liberality and public spirit of this gentleman in appropriating his own land to the different explorations, and in establishing at the Oxford Museum that splendid annex called by his name, in addition to another at Farnham, cannot be too highly praised, or too closely imitated.

Mr. George Clinch has brought to light at West Wickham, Kent, a large number of palæolithic and neolithic implements.

At Mount Caburn Camp, near Lewes, General Pitt-Rivers has conducted some important excavations, and his account of these, occupying seventy pages of *Archæologia*, is a model of minute analysis and lucid description. Few scientific tables have been constructed with greater precision. The archæological work of Thurnam, Sayce, Rolleston, Evans (father and son), Flinders Petrie, Condor, and many others, is of the same satisfactory character. The relics found at Mount Caburn, including bones of man and of the domestic animals, proved the camp to be a Keltic settlement of the late Bronze or early Iron age, directly preceding the Roman.

Mr. W. C. Borlase, the distinguished author of *Nænia Cornubiæ*, has examined more than two hundred sepulchral mounds in Cornwall alone, and unearthed abundant relics of the Metallic Ages. Among the pottery is the largest urn yet discovered in England, and it is of superior artistic shape.

The original existence of an avenue of double lines of stones leading up to the great circle of Stonehenge, has been established by the Rev. W. C. Lukis, and, whatever may be the design or meaning of this structure, it becomes now classable with the monuments composed of avenues and circles at Carnac and Stanton Drew.

At North Newbold, Yorkshire, in one of a group of barrows of the Bronze Age opened by Mr. G. W. Thomas, was found a skeleton of a man 6ft. 2in. high, a notable exception to the height of the broad-headed Keltic or Gallic people of the Bronze Age, which was 5ft. 8in.; the height of the small, dark, long-headed Neolithic people being 5ft. 4in., as well ascertained by Greenwell, Thurnam, Bateman, Dawkins and others.

At Freckenham, Suffolk, a hoard of gold coins—about ninety—has been discovered, all of a rare variety. It is known that the Iceni imitated the Macedonian stater of Philip II., but the present coins are deeply debased from that noble type. These coins are attributed to the reign of Prasutagus, the wealthy husband of Boadicea.* You remember, as I described it in a former paper, that the Ikeneld Street, so named from its leading to the country of the Iceni, ran, in its western branch, through Birmingham along the line of the Monument Road, and on to Sutton Coldfield.

Relics of the Stone and Bronze and Iron periods in Britain have been found in very many other places over a wide area. In the Orkneys, on the Yorkshire moors, in the Weald of Kent, in the Derbyshire limestone ridges, on mountain top, on sea-shore and in valley, in wood and cavern and morass, wherever they feasted and fought, mourned and triumphed, lived and died, our remote predecessors speak to us from camp and cairn and circle; from monolith and rock-shelter, and grave mound. Their weapons, their ornaments, their pottery, are familiar to us. Their fields of battle, their places of assembly, their earth-dug villages, are becoming more and more known. Their

* "Rex Icenorum longâ opulentiâ clarus," are the words of Tacitus.

struggle for existence, their forms of worship, their fashions of burial, have been brought into the clear light of modern investigation.

We understand better and better the surroundings which moulded and sustained them—the gradually lessening rigours of their climate, the growth and nature of their forests, the extent of their mining and their merchandise, their industries and arts, the animals on which they fed and with which they contended, the social relations, the habits and superstitions that influenced them, their very stature, shape and capacity of brain. Is not such knowledge of some use and interest to *us* in the close of the nineteenth century? Is it not profoundly true, as George Herbert wrote of modern man, that

> "All things unto his flesh are kind
> In their descent and being; to his mind
> In their ascent and cause."

And so in a different connexion, and perhaps even higher degree, do we not regard the memorials of far-off Eastern civilisation and primeval history? Gaze, for instance, on that wonderful statue of Princess Nefert, of the family of Seneferu, of the Third Egyptian Dynasty, in the Museum at Boulak, on the Nile—the calm, eloquent young face "with the look that in life it wore" full five thousand years ago, older than the Great Pyramid, but so like a Nubian girl's of yesterday. Or, again, examine the Chaldean record of the Deluge, the cuneiform inscription found by Mr. George Smith in the ancient Assyrian city of Erech—the most coherent, if not the only, inscription giving a version of an event mentioned in Genesis. Or yet again, consider the Moabite stone, inscribed nine hundred years B.C., in Phœnician letters, with the triumphant record of King Mesha's exploits, so confirmatory and complementary of our familiar Bible texts. Or, once more, bend over certain fragments of papyri in the Bodleian Library, and the skull and the tress of black hair found with them in an Egyptian cemetery, by Mr. Flinders Petrie, a year or two ago. That skull and that hair are those of a woman of perhaps Ptolemaic times. The

largest papyrus fragment contains the end of the second book of the Iliad in beautifully distinct characters. Who shall say that this lady, who loved her Homer so well that his writings were folded to her bosom in the grave, has no human interest for a generation which has produced an Elizabeth Barrett Browning, a George Eliot, and—it is no anticlimax—the distinguished classic graduates of Girton or Newnham?

Three remarkable movements bearing on prehistoric archæology in these islands must be mentioned with honour. First, the Bill for the Preservation of Ancient Monuments has become law, thanks chiefly to the unwearied exertions of Sir John Lubbock, to whom, by the bye, we owe the accepted names "Palæolithic" to express the period of the Drift, and "Neolithic" to express the polished Stone age. There have been scheduled most of the principal tumuli and megalithic monuments of the three kingdoms, and while all due reservations of the sacrosanct rights of property have been made, adequate measures have been taken for protecting the relics yet visible, and classifying them as objects of national value and national solicitude. Great extensions of it, however, are needed before the Act can be really effective in arresting the destruction still going on of our prehistoric remains of every kind. Many important monuments have yet to be scheduled.

The second of the movements I allude to is the commission given by the Society of Antiquaries to that distinguished archæologist, the Rev. W. C. Lukis, to investigate the prehistoric monuments of the country and make accurate plans, pictures and descriptions of them. A great part of his work has now been accomplished, and, when completed, it will be found to constitute the most faithful and comprehensive account of earth and stone structures ever presented to the world, the dimensions and drawings given being especially valuable. Such a work must do much to amplify our knowledge of the far remote settlements of man in Britain. For instance, although acquainted with most of the greater monuments at home and abroad, I knew little of Dartmoor, and was surprised to learn

from Mr. Lukis that in twenty square miles of it there is such a wealth of archaic remains. There are no fewer than twenty-four stone avenues, seven being of one row, fourteen of two rows, one of four, one of five, one of thirteen rows; ranging from two hundred to eleven hundred feet in length. But the Staldon avenue on the Moor exceeds everything hitherto known, being 11,239 feet long, and having at one end a circle of fifty-three feet in diameter, and at the other a cairn of twenty-seven feet in diameter. Mr. Lukis, indeed, describes Dartmoor as a veritable paradise for the student of prehistoric archæology.

He theorises modestly on the origin and design of the avenues, circles and cairns he has explored. He adopts for the most part the now widely accepted view that they are sepulchral —a view which is a violent reaction from the Druidism and mysticism of the seventeenth and eighteenth centuries. I believe, however, that the last or moderating word has not yet been said on this subject. It seems difficult to reduce to one uniform design the many dissimilar rude stone monuments scattered over Western Europe. Dartmoor may possibly be an imposing necropolis, but I hold, and have always held, that Stonehenge, Avebury, Stennis, and Carnac represent primarily some mode of *worship*, whatever burial-mounds may have been afterward raised within their precincts.

The third notable movement is the appointment of a committee of the British Association for "ascertaining" (to use the words of the instruction) "and recording the localities in the British Islands in which evidences of the existence of prehistoric inhabitants of the country are found." This very strong committee, headed by the genial author of "Prehistoric Times" and "The Pleasures of Life," has made two reports, and we may hope that fresh elucidations will result beyond the drawing-up of lists of objects and localities, essential though this be.

The recent traces of Rome in Britain, if not very numerous, are very striking. In three papers communicated to the Society of Antiquaries by the late Rev. J. Gerald Joyce, Rector of Strathfieldsaye, and in a supplementary paper by

Mr. Hilton Price, a singularly able account, with copious drawings, is given of explorations conducted by them, under the auspices of the late Duke of Wellington, on the site of the Roman city of Silchester, the most interesting of the Roman settlements in Britain. The discovery of several dwelling-houses and shops, of an extensive series of baths, and of the complete areas of the Forum and the Basilica, as also of a large number of coins, implements, ornaments, and memorials of various kinds, has extended materially our knowledge of the domestic life of Imperial Rome in Britain.

Another important investigation is that of Mr. Alfred Tylor in Warwick Square, London. He deduces from certain objects there found some new conclusions as to Roman London, as to the state of Britain in the Claudian age, and as to the Mithraic character of many Roman symbols.

A still more valuable discovery is a large portion of the Roman wall on the north side of the old city. This was made last year during the extension of the General Post Office. I believe that the authorities have willingly agreed to maintain the wall uninjured and accessible.

Two remarkable Roman graves have been opened at Sittingbourne, and an unusual collection found of glass bottles, bronze vases and Samian cups. So also at Dorchester.

Mr. G. Esdaile, in an able essay on the Roman Occupation of Britain, has shown that we should expect to find at every spot where *chester* is part of the name, a fixed area of encampment having some approximation to the area of about 86 acres (2320×1620 ft.) laid down by Hyginus in the first century, as the proper form and size of a camp established before a besieged place. And Mr. Esdaile proves that this is so in regard to twelve English towns, formerly early British settlements, and that the position of the gates, where traceable, accords with the plan of Hyginus.

The logic of the pick-axe dispels as well as confirms many venerable traditions. The so-called Cæsar's Camp, at Folkestone, has been explored by General Pitt-Rivers, who found

nothing Roman in it, but only relics of mediæval times. It was the fashion to call any earthwork in the south of England by this great name, *Omne ignotum pro Cæsare est.*

Every year adds to the list of Roman inscriptions obtained by accident or diligence. Chester and the counties north of it are the most fruitful area. Several of our antiquaries devote themselves to the collection and translation of these often fragmentary and often obscure inscriptions, and we shall have in time an interesting *corpus* of Roman Epigraphy in Britain, to be compared with the standard foreign records.

The Isle of Wight, which became a Roman possession in the first century, has yielded a very fine example of a villa at Morton, near Brading, which surpasses the villa discovered at Carisbrooke in, I think, 1856. It is noteworthy that although the Roman occupation of Vectis lasted at least three hundred and sixty years, so few memorials of it have come to light, and that these are all subsequent to 1816, when Sir Henry Englefield wrote his history of the island. At Morton an extensive ground plan has been developed, and I do not know that the whole area of the villa has yet been explored. Up to 1884 thirty-six chambers had been examined, some of them being of extreme interest. Elaborate and beautiful mosaic pavements, and fragments of frescoes were uncovered. The subject of Orpheus attracting the woodland beasts by playing on the lyre, a favourite one in Roman decorative work, is seen on these pavements, with the addition of one animal, the monkey, not found at Woodchester, Winterton, or other Romano-British villas. There is also in a kind of picture gallery, 40 feet long, a remarkable set of mosaics, among which are three other favourite classical subjects, the Four Seasons, Perseus and Andromeda, and the Head of Medusa. This last is a very frequent theme in ancient art. It appears on numerous walls and pavements, and on many imperial coins and gems. It was often used as a symbol of sorrow or despair. The most beautiful example I have seen of the Gorgon head is in the possession of Dr. John Evans, who exhibited it in 1887.

This is an onyx cameo of large size, and of extreme grandeur and delicacy, the work, probably, of a Greek artist. It was found in the Tiber at Rome. In the Medusa chamber at Morton there is a striking figure of a man seated, surrounded by astronomical emblems or instruments. He is supposed to be Hipparchus or Pythagoras.

From the well of this villa the skeleton of a man was taken up, with bones of the ox, deer, and other animals. The Morton villa must, I think, have been occupied during a very long period, perhaps from early in the third century down to Saxon times, as some portions of it belong to later periods than others, and as coins ranging from Domitian to Honorius, over three hundred years, have been found.

Lastly, I would mention the famous Roman Baths at Bath, which have been for the most part exposed within the present decade. It is unfortunate that, despite the strenuous remonstrances of the Antiquarian Societies, these striking remains have been in some measure obscured or obliterated by the treatment applied to them, so that the fine circular and rectangular baths, the hypocausts, tanks, chambers and corridors, can only be imperfectly studied in their new coverings and surroundings. Much of the Roman lead, and the Romans were never sparing of that metal, has been stripped and sold for modern gutters. It is gratifying, however, that during the meeting of last year in Bath, the British Association urged the civic authorities to make further researches, and to guard carefully the remains discovered; and that to this end the council voted a sum of £100 to the Corporation.

An interesting and clearly written work has been published by the Rev. Francis Vine, called "Cæsar in Kent," in which the question of Cæsar's landing place, and of the sites of his battles with the Britons, is amply discussed. Nothing has raised greater controversy than this question of the landing on the first invasion. (On the second invasion the place was the same.) Antiquaries, astronomers, and nautical men have laboured at the point with painful minuteness, but it cannot yet be said to

be determined. Halley and Sir G. B. Airy differ *toto cælo*, the one fixing Deal, the other Pevensey Bay. Dr. Cardwell is for Deal, as are also the late Emperor Napoleon and Mr. de Saulcy. Mr. Thomas Lewin believes in Hythe, Archdeacon Batteley and other authorities in Richborough. Mr. Vine decides for Deal. It is agreed on all hands that Cæsar anchored off Dover Cliffs at ten o'clock on the morning of the 25th, 26th, or 27th August—the exact day depends on the true sense of his phrase, "post diem quartum," in reference to the full moon*—and that he left the anchorage at three p.m., when both wind and tide were in his favour. But the difficulty is, did the tide at three o'clock carry him eastward or westward of Dover—up Channel or down Channel? If the former, Deal or Richborough would probably be the landing-place; if the latter, Pevensey or Hythe. The Admiralty having made prolonged researches into the ebb and flow of the tides on the days in question, in that part of the Channel, favour the latter supposition. Cæsar, by tidal evidence, must have moved to the west.

The evidences from camps and relics are not conclusive. Romney and Hythe can show more of interments of men fallen in conflicts on the coast, but the line of camps and fixed stations from Deal through Canterbury, Rochester, and Dartford, to Keston and Walton-upon-Thames, better supports the theory of Deal as the landing-place, and to that I myself certainly incline. The greater merit of Mr. Vine's book is the able description of the Roman march from the sea through Kent, and across the Thames to Verulam where Cassivellaunus was defeated.

In the Anglo-Saxon period a few notable discoveries should be named. At Deerhurst, near Tewkesbury, close to the celebrated pre-Norman Church, the relics have been brought to light of a small chapel attached to, or rather subjacent to, a building once known as the manor house of Deerhurst. The chapel is of the middle of the eleventh century. It has a

* The full moon was on the 31st August, at three a.m., B.C. 55, or A.U.C. 699.

chancel-arch and doorways, and in the nave two very interesting double-splayed, round-headed windows. An inscribed fragment of stone was found, recording the dedication to the Holy Trinity. A stone with a similar, but more complete, inscription had previously been removed from a site near the Church. *This* stone is in the Ashmolean Museum at Oxford. A visit to the little Saxon chapel and to the sixteenth century house built over it, would well repay you, even after seeing the unique cruciform Church of St. Lawrence, at Bradford-on-Avon, whose date is 705. Anglo-Saxon cemeteries, with abundant relics, have been discovered at many places within the past ten years; for example, at Sleaford, at Sittingbourne, in the cricket field of St. John's College, Cambridge, and at Wheatley, near Oxford, which last I described in a paper read to this Society in 1884. Little of material import has been added by these explorations to our knowledge of Saxon communities and customs. But at Taplow, in Berkshire, a vast barrow, with an undoubted A.-S. interment, has been examined. It was of exceptional size, 13ft. high and 250ft. in circumference, and the ornaments in it were both valuable and rare, including —and this is the solitary example known—a silver-mounted drinking horn. Another Anglo-Saxon cemetery has just been discovered (October, 1889) near West Shefford, Berkshire. About thirty skeletons of both sexes, with the usual arms and ornaments, were exhumed.

As regards Scandinavian relics, a few Danish earthworks have been examined, and one or two boats of the Vikings unearthed, but nothing found has warranted *pro tanto* the remarkable theory of M. Paul du Chaillu in his recent paper at Newcastle. If the Viking element *be* dominant in the A.-S. stratum of our nationality, at least there is no proof of it in the living speech, no hint of it from underground.

The growth and prosperity of the museums in which many British archaic remains are preserved is very gratifying. I do not speak of our National Museum, which, by the steady accretion of years, has become the centre whereto gravitate all

the new discoveries of the Old throughout the world; nor yet of such noble institutions as the University Museum at Oxford, dedicated to the Natural Sciences; but rather I allude to such collections as the Ashmolean, of purely antiquarian character. Miss Celia Fiennes, toward the end of the seventeenth century, describes it as "a little building w^{ch} is full of Antiquityes, w^{ch} have many Curiositys in it of Mettles, Stones, Ambers, Gumms." * The efforts of several distinguished Keepers, from Edward Lhwyd to John Henry Parker, have made it much more than *that*, and under the recent patronage of the University and the management of its present accomplished Keeper, Mr. Arthur J. Evans, it is becoming an establishment truly creditable to archæology and not unworthy of Oxford.

Attention has been drawn by Mr. De Gray Birch and others to several unpublished charters of A.-S. times, and to the necessity of collecting and collating all the known charters, perhaps two thousand in number. In fact, a new edition of Kemble's great work, the "Codex Diplomaticus," corrected and amplified to the present time, is most desirable in the interest of national history, topography, and genealogy. Meanwhile, the Bishop of Oxford, Dr. Stubbs, has worked—how ably students well know—at a copious selection of Latin and Saxon charters and laws. His book is become a University text-book of the highest repute.

Mr. G. Laurence Gomme has laboured in the field of Deduction, and, basing his conclusions on the large data available concerning the Keltic, Roman, and Saxon settlements in Britain, he has contributed to our literature three valuable essays: (1) "On Archaic Conceptions of Property in Relation to the Laws of Succession;" (2) "On Traces of the Primitive Village Community in English Municipal Institutions;" (3) "On the History of Malmesbury as a Village Community." Mr. Seebohm has treated this subject with still greater fullness and from a different point of view as to allodial and other tenures.

* "Through England on a Side Saddle," p. 25.

The collection and publication of the complete body of chronicles and memorials of Great Britain, preserved in the National Archives, is an object that has long occupied the attention of the Society of Antiquaries of London, and that has been pressed by it on successive Governments. The immense value for historical purposes of records of the old proceedings in courts of Law, of Chancery Rolls, State Correspondence, and reports and abstracts of many kinds, can hardly be questioned, ranging as they do from the twelfth to the present century. There is little doubt that the whole series will some day be accessible in a collected form. Meanwhile we have the excellent publications of the Record Commission and of the Pipe Roll Society. These include the great Roll of the Pipe, that is, the Accounts of Receipt and Expenditure of the Revenue, from 1130 to 1165, but how small a portion is this of the records which are available year after year—two years only excepted—down to the present time !

There have, however, been printed by the William Salt Archæological Society in the collections for the History of Staffordshire, the Pipe Rolls relating to this county for 1130, and for the period 1155-1216, together with many other valuable Rolls and historical papers.

The better preservation of Parish Registers, which date from 1536, and the publication of them in several instances, is surely an increasing necessity. The same may be said of older episcopal registers and Church documents in the keeping—not always the *safe* keeping—of Deans and Chapters. When we get our Bishop of Birmingham I hope he will make a fair start in this direction for the benefit of, say, the twenty-ninth century.

Court rolls of manors are often of especial value and interest, yet they are shamefully neglected, as Professor Chandler has pointed out. He mentions that the authorities of an Oxford college sold for thirty shillings a cartload of deeds, which the Bodleian would gladly have accepted. And he himself purchased for a few pence from a marine-store-dealer a

number of deeds relating to Coventry and its neighbourhood, ranging in time from Henry IV. to George II.

And our old Municipal records, too, are often sadly neglected. Could they be more fully and more generally transcribed by competent hands, and published at prices not prohibitive, how much light would be thrown on the growth and development of our civic life and our national institutions!

Something, however, in this direction has been accomplished by the series of "Popular County Histories" and of "Historic Towns," the latter being under the editorship of Dr. Freeman.

I am looking forward with confidence to the undertaking of the Statistical Section of this Society, under the inspiration of Mr. Bunce, to compile, from our city archives and other sources, a new municipal and industrial history of Birmingham. Dr. Maunde Thompson, the principal librarian of the British Museum, said truly, at the meeting in London of the Library Association last Wednesday, that the first duty of a free library is to collect all local literature, and all manuscripts, deeds, and records of local interest. The British Museum, he stated, possesses 60,000 of such documents under the head of "Charters and Rolls," which are freely accessible to enquirers, and could, in some cases, even be purchased by a local Authority.

Let me sum up all that can be said on the deficiencies glanced at, by pleading for the general Archæological Survey so often proposed by our best antiquaries and scholars. This Survey would show how the foundations of the Present are laid deeply in the Past. It would search out, analyse, and co-ordinate all the monuments, literary and material, scattered over England, Scotland, and Ireland, throughout historic and pre-historic times. And thus it would be a national work at least equal to that already in very great measure achieved by France and Germany, and even by Denmark. Mr. George Payne has made a good beginning, and set an excellent example, by his "Archæological Survey of Kent," which is illustrated by a carefully drawn map. (1888.)

I am glad to say that, with this end, and generally for the better organisation of Archæological research, a conference of the leading societies in England and Wales was held in London, in last November. The final reports and resolutions are not yet issued. Meanwhile, the ardour of private historical enquiry has been unchecked by any disabilities. Students of particular epochs, critics of particular men and events, have found means of access to the documents they desired. Dates have been corrected, obscurities removed, new theories asserted, and new readings enforced. Controversy has waxed warm as to the tombs of Christopher Columbus and Alexander the Great. Some amusing, if not always successful, rehabilitations of character have been attempted. Tiberius, Catiline, Clodius, William I., Edward I., Richard III., Henry VIII., Mary of Scotland, Marat, and Robespierre, have in their different degrees been more or less exalted, one or two of them even apotheosised. It is an age of demolition and doubt. The spirit of Niebuhr moves among us. King Arthur is an imposing myth. There was no Robin Hood. There was no William Tell. Edward II. was not murdered in Berkeley Castle or anywhere. The Earl of Leicester was guiltless of the death of his wife, Amy Robsart. Are we, since Whateley, quite certain about Napoleon and Waterloo?

I have alluded to the mutilation and destruction of a particular class of documents and monuments, but all this is as nothing when compared to the obliteration of ancient monuments and architectural remains, by the process called "Church Restoration." Last year the Society of Antiquaries of London, which had often striven to prevent mischief being done in the name of restoration or repair—St. Alban's is a notable instance--addressed a spirited remonstrance to the dignitaries of the Church throughout England. I quote the following words:—"On visiting a 'restored' church, it is found that monuments and painted glass, of which the existence is recorded in county histories, have not only been removed from their original positions, but are no longer forthcoming; that

inscribed slabs from tombs have been used to bridge over gutters, or to receive hot air gratings, or have been covered with tiles; that the ancient fonts have been removed, the old Communion tables destroyed, the Jacobean oak pulpits broken up or mounted on stone pedestals, and not unfrequently the old Communion plate sold. The architectural features and proportions of the churches have in innumerable instances been modified, especially so far as regards the east windows and the character of the chancels generally." And again: "The Society does not overlook the necessity of adapting the buildings to the wants of the present day, but it contends that the greatest part of the mischief that has been done to our churches has not added to the convenience of the buildings, which is in no way aided by destroying the more recent portions of a church, and re-building them in a style which imitates the older portions, nor by the destruction of furniture and monuments, only because they are not of the date which is assumed to be that of the church. New work done to suit new wants, and not pretending to be other than it is, will carry on the history of the building in the same manner as did the old, and the Society has no wish to prevent that from being done. It only urges that the ancient record should not be wiped out to make room for the new, nor falsified by making the new a servile imitation of the old."

In this connection I wish to submit to you that one of our most interesting and venerable buildings, the Benedictine Abbey and Church of Croyland in the Fens, is now in imminent need of repair and *judicious* restoration. Its history dates from 716, when King Ethelbald founded it. It has been several times burnt and otherwise injured. The Rector of Croyland, the Rev. T. H. Le Bœuf, and the architect, Mr. John L. Pearson, have done all they can with the funds at their disposal, but £3,000 more is required. The safety of much of the structure is still endangered. Croyland Abbey claims to be the nursing Mother of Cambridge University. Will not the Cambridge graduates of Birmingham come to her aid?

Foreign Governments—especially the Italian—have set us of late years more than one example of faithful and discreet restoration. The renewal of the Palace of the Doges at Venice, now almost completed, has not changed a single feature or suppressed the smallest detail of that matchless edifice.

It has been proposed to enlarge Westminster Abbey so as to receive the remains of our illustrious dead in future ages. We are all agreed that this should be done, but the extreme difficulty is to add a new chapel to the fabric without interfering with the great cloister, or with the nave, or with Henry VII.'s Chapel. Mr. Shaw-Lefevre has presented a Bill to Parliament to give effect to his latest plan, and it is certain that the best endeavours of all concerned will be applied to afford the needful relief to our overcrowded Abbey, and yet to pass it on unmutilated and unvulgarised to future generations.

It is satisfactory to know that historical archæology has of late years become a favourite study in our leading societies. That which bears on the making of a nation, on the moulding of its character, on the course of its fortunes; that which exhibits it in war and peace, explains difficulties, illustrates epochs, sets men and events in clearer view; must ever be of greater interest than aught beside. Antiquarianism will decline among us if it concern itself only or mainly with isolated objects, however curious or rare, that tell no tale and have no human relevance. The collector of archaic odds and ends—of dubious fossils from the hill-side, of classic coins from the dealer's shop, of pilferings from unknown obelisks, or relics from unlocalised graves—can take no rank with archæologists of culture and discernment. I do not, of course, speak of collections for a particular purpose, such as sets of gems, or rings, or seals—a handsome volume on the Great Seals of England has been published by Messrs. A. B. and Allan Wyon—or of those splendid exhibits recently made at Burlington House, by Mr. St. John Hope, of mediæval drinking bowls and trenchers, of Corporation maces and swords, which so well illustrate the old civic and social life of our country.

In the same spirit I would refer to such researches as those of the Folk-Lore Society, which may be of great interest and value, or may degenerate into mere triviality.

I would call your attention for a moment to the *practical* side of archæology, that is, in the modern sense of practical. It is undeniably to the labour of the antiquary that we owe those matchless specimens of Greek and Etruscan art which delight our eyes and inform our understanding in the museums of the land. It is to the antiquary that we owe the transmission of Roman law and language which count for so much in our own, and not less the example of the massive Roman bridge, and the unbending Roman road. Education and the Arts would surely be impoverished could there be withdrawn the writings and the material works of the ancients that have been handed down through the centuries by cloistered student and enlightened ruler.

And so the more recent enquiries into the elements of our living language are due to a small and unobtrusive company of workers in the mines of past ages—mines often left as exhausted by older hands. Anglo-Saxon vocables are better understood now than in the time of Thorpe and of Bosworth. Middle English has been studied as it never before was. Caedmon and Chaucer are text-books in our High Schools. Glossaries of provincial words have been multiplied. A Chair of Keltic Literature has been founded at Oxford. The Etymological Dictionary of Professor Skeat has enlarged our comprehension of the English tongue. I had the privilege a few days ago to examine, in the Scriptorium of Dr. Murray, at Oxford, the progress of the new Dictionary which he is preparing under the auspices of the University, and I was struck with the amount and the depth of antiquarian research he is applying to this truly great production. And again, such books as Isaac Taylor's "Words and Places," Dr. Mitchell's "Past in the Present," Dr. Anderson's "Scotland in Early Christian Times," Mr. Green's "Making of England," and Max Muller's "Lectures on the Science of

Language," may be described as the work of literary archæologists of the highest rank.

Many contributions to Art, Literature, and even Science have indeed been made by Archæology during the past few years, throughout the four Continents, but I must not now enter on so wide an area. One or two other examples I may mention. The first dates so far back as 1876; it is embodied in a treatise of Mr. James Fowler on the "Process of Decay in Glass." The paper, occupying a hundred pages of *Archæologia*, is a comprehensive and minute elaboration of its subject. It traces glass from the first employment of it in Egypt, 2500 years B.C., down to our own time, through its varieties of form, colour, and transparency; through the characteristics and causes of its degradation and decay; through the modes of its use from cottage to cathedral; through the manifestations of it from the beads in mummy-cases and in prehistoric barrows, to the many-tinted table glass of our dining-rooms, and the glorious windows of Fairford or King's College Chapel. The paper is an excellent example of the value of chemical and mechanical knowledge in an archæological study, and it is full of helpful suggestiveness for those who are concerned in making or in using glass in the present day.

To the same antiquary, Mr. James Fowler, we owe an examination of the effect of *gas* on painted glass, on metal, stone, bookbindings, and pictures. He has exhibited specimens of leather from the books of the Registry of Deeds Office at Wakefield, and of stone from the crypt of York Minster, and from the tomb of Archbishop Savage in the north aisle of the choir. In these specimens, crumbling and perishing to the sight and touch, the mischievous action of sulphuric acid from common gas has been abundantly proved, and similarly with the beautiful screens and windows of many churches. Surely here is a new argument for the electric light!

As another contribution by Archæology to Art, I would name the very valuable book of Miss Margaret Stokes on "Early Christian Art in Ireland." This embraces the Illumination,

Metal Work, Sculpture, and Building Architecture of seven centuries, from the fifth to the twelfth. The book is admirably written and illustrated, Miss Stokes being herself a true artist, as her fac-similes of the Book of Kells show, and her work forms one of the choicest of the South Kensington handbooks. Her practical object, which is the object of all sound archæologists, is set forth as being to indicate how far the knowledge of the arts of Ireland in the past may subserve their higher development in the future. She does not, to use her own words, desire "to present a guide for the antiquities of Ireland, but rather to indicate how these antiquities should be approached so as to draw forth whatever elements of instruction may lie hidden in them for workers in the present day."

In the same spirit have Miss A. W. Buckland and Miss Amelia Edwards worked in their several departments of archaic study, and also Miss Harrison, with her wonderful knowledge of Greek art and letters. I would add the highly-gifted Miss Ramsay, or rather Mrs. Butler, to the list, and other names will occur to you. Considering such minds as these, is it not a marvel that our old learned and scientific societies should continue to exclude women from fellowship? They have the electric light in their halls, but it does not penetrate the mediæval mists of custom and prejudice.

We have certainly made *some* advances since the Council of Nicea, A.D. 325, when it was gravely debated whether woman had a soul; and since the Council of Macon, in the fifth century, when a priest essayed to demonstrate that she was not properly of the human race. The assembly decided by a small majority that she *was*. But much remains to be done, and, while we admire, as antiquaries, the civilisation of Egypt and of Greece, let us remember the higher place in it occupied by woman, and how inferior is our civilisation in that regard. I am not now speaking of her political subjection among us. John Stuart Mill and twenty years of discussion have taught us the truth about *that*, but I am speaking of intellectual fields where woman may meet man on equal terms on common

ground—ground which, indeed, she is occupying with more and more confident demeanour and resolute tread. It is a pleasure to me to know that in the Birmingham Philosophical Society we have always wished for and welcomed the membership and presence of ladies. I am most glad that a lady—and one of such educational distinction—is on our new Council, and that there is no reason why a lady should not become our Secretary or our President.

I must also, as an antiquary, congratulate the Society on its adoption of Historical Studies among the subjects of its papers. I need not dilate on the inter-connexion of Archæology, Anthropology, and History, or on the relation of them all to Science. We have men in Birmingham who are living examples of what I mean. These men raised from its ashes, in the sight of admiring England, ten years ago, the great Free Library, in which had been stored so ample a collection of works in the branches of knowledge I have named. And these men, with others, by their unwearied efforts, have made the Library more splendid than ever. Birmingham, not for the first time, refused to be disheartened. It is the old unquenchable spirit expressed by the father of Latin poetry:—

> "Quæ neque Dardaniis campis potuere perire,
> Nec cum capta capi, nec cum combusta cremari." *

I regret—we must all regret—that the Historical Society, so ably constituted and so auspiciously inaugurated nine years ago, has ceased to exist; but if it be merged into our larger Society and flourish as one of its sections, it may yet have an honourable and useful career. Many of us remember the words of its first and only President, Dr. Edward Freeman, words with which I will conclude my own address, as being indeed the keynote of what I have had the pleasure to submit to you.

"We believe," said Dr. Freeman, "in the true brotherhood of Sciences. We believe that he who depreciates any one among them does no real honour to the other which he tries to

* Ennius, *Annales*, Lib XI., 393.

exalt. . . . . We know that the study of man constantly needs the study of matter as an equal friend and companion. We, whose study is political history, the history of mankind as members of civil communities, feel no slight tie of brotherhood toward those who teach us the history of man's home, the earth. before man arose to take possession—towards those who teach us the history of the earlier forms of animal life which were before man, or against which man had often to struggle—toward those who teach us the history of the lower forms of man himself, and who put us in the way of tracing the steps by which, out of such rude beginnings, civil society could shape itself into the democracy of Athens, the kingdom of England, the federal commonwealth of America."

II.—*On the Effects of a Reputed Waterspout at Batcombe and Chetnole, Dorsetshire.*

By Rev. G. Deane, D.Sc., B.A., F.G.S.

[Read before the Society, February 5th, 1890.]

It may be in the recollection of the members of this Society, that on the 8th November, 1888, I gave an account of the effects of a so-called waterspout at the Wittenham Hills, Berkshire, an abstract of which has been published in our "Proceedings" for last session. In that paper I ventured to dissent from the popular notion of a waterspout; and explained the phenomena by enormous rainfall caused suddenly through electrical disturbance, and aided in the erosive power of the water by the configuration of the ground.

On the 7th June, 1889, a somewhat similar excessive concentration of thunder rain occurred on the Chalk Downs of Dorsetshire, at the little hamlet of Batcombe, about midway between the towns of Dorchester and Yeovil. The resulting flood in the valley between Batcombe and Chetnole on the

north-west side of the Chalk Downs was very great; and much damage was done to both property and live stock, accompanied in some instances by danger to human life. The south side of the hills did not suffer so much; but there also was considerable flood and consequent erosion. Popular attention was awakened, the waterspout theory was broached; and the local newspapers as in duty bound, gave full accounts of the matter. The *Western Gazette* of June 14th contained a somewhat sensational description of the damage done, and adopted the waterspout theory. A similar though shorter account appeared in the London *Daily Chronicle* of June 10th. But the local *Western Chronicle*, with more caution, discredited the waterspout theory. Other newspapers, doubtless, recorded the facts, but these three are the only ones which have come to my hands.

Not content with the newspaper accounts, and unable to visit the spot within any reasonable time after the occurrence, I put myself in communication with the Rev. H. J. Poole, of Stowell Rectory, near Sherborne, who is well known in the district as a meteorological observer. He was good enough to send me copies of photographs taken by himself, and to give a detailed account of what he himself witnessed a few days after the flood. Extracts from his letters will make the facts clear. After describing the havoc wrought at and near Batcombe, he continues:—"I then proceeded along the road to Cross-in-hand and thence to High Stoy. The whole of this road from Cross-in-hand is cut in the north-west side of the hill, and looks down on the combe below. Here there were numerous 'washes' in the road, and nearly at the summit of the hill occurred the heaviest 'washes' in the whole course of the flood. There were pits, then filled in, but 8ft. or 9ft. deep, as I was told by the roadmen; the course of the wash to some 7ft. to 8ft. above this level to the top of the hill was only moderate, but plainly discernible. I could trace it to the cross roads just on the top or plateau, 7ft. or 8ft. above the level of the deep pits, but there was not the slightest indication at this point as to where the immense mass of water capable of excavating ground some 8ft.

could possibly have issued. I made every search for traces, but the roads around had not been in the least disarranged, nor could I see any marks on the grass. The summit or plateau at this point is distinctly visible from Upcerne, where the eye witness of the waterspout resides, but I could not learn his name.

"The curious thing is that the deepest excavations occurred nearly at the summit or plateau, where one would expect the least force from a heavy rainfall.

"I then drove down the combe in the direction of Cross-in-hand, photographing view to north-west. This overlooks the valley along which the united downpours from West Hill and High Stoy flowed in the direction of Chetnole.

"I witnessed the damage done at Calfhay, Withyhook Mill (where a waggon was washed away 1½ miles), and Heniford Mills, where the miller told me the water came down in ten minutes, and rose 15in. higher than the highest previous inundation in 1882. I then visited Col. Digby's residence at Chetnole, where the water threw down a brick wall 10ft. high and 30ft. long, with very little derangement of the bricks. The greenhouse also was turned over.

"On the whole I think your theory explains the phenomena, with the exception of the very deep excavations at the summit of High Stoy. I shall make further enquiries as to the eye witness of the 'spout' at Upcerne."

Such is the account by an observer of what he saw a few days after the catastrophe. I may add that in December last I met Mr. Poole at a lecture which I gave in the neighbourhood on "Waterspouts," and ascertained that, after diligent enquiry, he had utterly failed in discovering the eye witness of the "spout" at Upcerne. That redoubtable gentleman appears to have no real existence; though if he does exist what he saw was, doubtless, a vortex of whirling cloud. (See a letter signed W. K. Burton, in "Nature" for November 7th, on "Electrical Cloud Phenomenon," vol. xli., p. 10.)

Now I come to my theory of the matter. The hamlet of Batcombe lies at the head of an almost semi-circular *cul-de-sac* of hills opening northwards. From High Stoy on the east to Bubb Down on the west, which, so to speak, form the horns of the crescent or semi-circle, is a distance of $3\frac{1}{2}$ miles, and the line of hills recedes southwards about $1\frac{1}{2}$ miles, having Batcombe at the apex. This line is broken by the usual combes occurring in chalk districts, the principal of which are between the East and West Hill at Batcombe, and between the East Hill and High Stoy. Above these on the south there is the usual "down" or plateau. The Upper Greensand and Chalk strata here rest on the impervious Oxford Clay, the intervening strata being absent. Sudden and enormous thunder rainfall over this area must result in copious inundation in the Chetnole valley. One flood would sweep down from the West Hill of Batcombe, and another from the combe between East Hill and High Stoy; and, joining their forces, would carry destruction to the valley below. The remark of the miller at Heniford shows that inundations are not uncommon; and the only peculiarity of this one is that it was more rapid, and rose 15in. higher at that point than his previous highest record in 1882. Electrical disturbance—it may be the blending of two thunderstorms—on the hills above, accompanied by excessive rainfall, is all that is requisite to explain the phenomena on the slopes of the combes and in the valley. The large semi-circular *cul-de-sac* of uniting valleys would speedily concentrate the flood upon the unfortunate folk below. There is no need of a "waterspout" to explain this.

There remain, however, to be accounted for the deep pits by the road near the summit of High Stoy. These were two in number, and were from 12ft. to 14ft. by 6ft. in area, and 8ft. or 9ft. deep.

There appear to me to be at least two possible simple causes of these "washes" near the summit. It is well known that most of the chalk downs are capped by a layer of porous flint gravel, often of considerable thickness, which is the débris

or detritus resulting from solution of the chalk by carbonated rain-water. The excessive rainfall on the plateau above would readily find its way through the porous gravel, and would easily acccount for roadside erosion at a level of 7ft. or 8ft. below the summit.

Or, it is equally well-known that chalk surfaces contain cracks, crevices, and pot-holes, through which such rainfall would rapidly pass, and occasion the washes in question. My correspondent, Mr. Poole, states that he saw no indication of anything of this kind; and has "no doubt whatever that the excavations were made by a vertical impact of water or some other body, and that within the very narrow limits of four or five yards." It is unfortunate that these two pits were filled up before they could be carefully examined, as most probably their shape and their sides would have given distinct proof of their origin. Nevertheless, chasms 14ft. long, 6ft. wide, and 8ft. deep point clearly to the vertical erosion of running water.

I have taken an interest in these so-called waterspouts on account of the widespread popular misapprehension on the subject. If facts can be explained by simple and ordinary causes there is no need to seek the intervention of what is extraordinary and problematic. At the same time it must be admitted that the scientific explanation of cyclones, tornadoes, waterspouts, *et hoc genus omne*, has in recent years undergone considerable change, and may now be described as in a state of flux.

I do not know what may be the popular conception of a waterspout. But a column or spout of continuous water, kept as such by a whirling wind (without which, of course, it would divide and scatter), if descending from much altitude, would fall with great impact; and, provided it remained stationary, would cause much greater excavation than 8ft. or 9ft. Such pits are too shallow for such a theory. Moreover, all known waterspouts have a motion of translation; and there is distinct evidence in this case that no such motion of translation

occurred. Further, observed waterspouts are vortices of cloud or water vapour, not continuous columns of water; and, when they burst, as it is called, the result is torrents of rain, not a spout of water.

The old theory of circular storms, whether cyclones of a thousand miles in diameter or tornadoes and waterspouts of as many inches, is that of the ascent of a column of air, started by unequal temperatures above and below, and maintained in continued ascent by the latent heat given out through condensation of its water vapour, and thus originating vortex movement by the impact of the air rushing in below from all directions. But many objections have been made to this theory, and of late years, the "eddy theory" of M. Faye,* of Paris, has been coming to the front. According to this theory, circular storms are simply eddies in the higher streams of air which pass from the equator to the poles. Such eddies, by gravitation and centrifugal force, naturally descend, thus affecting the lower strata of the air, and giving rise to cyclones, tornadoes, and waterspouts. There is much to be said in favour of this theory; and if it be true, it is, of course, possible that one of such aerial eddies touched at its lower extremity the high Chalk Downs of Dorsetshire, and brought down aerial streams of condensed vapour; though, even then, this would fall in torrents of rain, and not as a column or spout of continuous water.

Although this may be possible, on M. Faye's theory, the whole circumstances of the case taken together appear to me to point to ordinary thunderstorms of exceptional violence as the cause of the Batcombe and Chetnole cataclysm. The watery effects at Batcombe, distant two miles from High Stoy, and at Ailwell, distant more than three miles, were (with the exception of the two deep erosions) at least as great as at High Stoy; thus showing that the pits at the latter place were rather an incident in a general torrential rainfall than the cause of the

* Sur les Tempêtes: Théories et Discussions Nouvelles. Par M. H. Faye. Paris: 1887.

whole. It would seem not improbable that an upward discharge of lightning from the summit of High Stoy was responsible for the concentration of rainfall there, and the origin of the roadside erosion forming the deep pits. And though I am strongly inclined to the theory of M. Faye, as to the cause of cyclones, tornadoes, and other circular storms, I believe that the true explanation of the Batcombe and Chetnole cataclysm is the one given in this paper.

III.—*Examples of "Solution-compounds."*

By G. Gore, LL.D., F.R.S.

The term "solution-compounds" has been applied chiefly to those substances which exist only whilst dissolved in water, etc., and are decomposed on evaporating or crystallising the liquids. A method of detecting those bodies and of ascertaining the combining proportions of their ingredients by means of the "voltaic-balance" has already been published (see Roy. Soc. Proc., Vol. XLV., p. 265).

The formula of each "solution-compound" in the following series of examples is the one yielding the smallest amount of voltaic energy, and is indicated by a star (*). The act of chemical union is usually attended by a great loss of such energy, and the relative amount of voltaic energy of the new compound is usually very much smaller than that of the mean of its constituents. Each "solution-compound" is recognised by a depression of such energy. In each of the following cases only a sufficient number of measurements are given to show a portion of the depression of energy caused by chemical union of the

ingredients of the compound. One example only is given of each class of compounds, and only average amounts of available voltaic energy are stated; the proportions of such energy given do not include the amounts lost by "local action." Zinc-platinum couples and aqueous solutions were employed in all cases.

Examples.

1.—*Element with Element.*

CHEMICAL EQUIVALENTS.		AVERAGE ENERGY.
4 Br+5 Cl	at 20° C.	1,934,496
4 ,, +4 ,, *...	,,	1,817,901
4 ,, +3 ,, ...	,,	1,981,035

2.—*Element with Monobasic Acid.*

4 HCl+5 Cl...	at 21° C.	863,774
4 ,, +4 ,,*	,,	739,922
5 ,, +4 ,, ...	,,	876,300

3.—*Element with Bibasic Acid.*

2 H^2SO^4+5I	at 18° C.	320,460
2 ,, +4,,*	,,	267,958
2 ,, +3,, ...	,,	312,815

4.—*Element with Tribasic Acid.*

Citric Acid+4 Br	at 20° C.	776,944
,, +3 ,, *	,,	636,956
,, +2 ,, ...	,,	706,506

5.—*Element with Tetrabasic Acid.*

$H^4P^2O^7$+5 Br	at 20° C.	716,768
,, +4 ,, *	,,	586,785
,, +3 ,, ...	,,	614,269

6.—*Element with Monobasic Salt.*

4 Am Cl+5 Br	at 17° C.	355,698
4 ,, +4 ,, *	,,	308,699
3 ,, +4 ,, ...	,,	326,081

CHEMICAL EQUIVALENTS.		AVERAGE ENERGY.
7.—*Element with Bibasic Salt.*		
2 K^2SO^4+5 Cl	at 20° C.	74,368
2 ,, +4 ,, *	,,	50,648
2 ,, +3 ,, ...	,,	54,536
8.—*Element with Tribasic Salt.*		
2 Na^2HPO^4+7I	at 21° C.	30,869
2 ,, +6,,*	,,	26,556
2 ,, +5,, ...	,,	28,703
9.—*Element with Tetrabasic Salt.*		
$Na^4P^2O^7$+5 Br	at 21° C.	104,387
,, +4 ,, *	,,	82,289
,, +3 ,, ...	,,	86,377
10.—*Monobasic Acid with Monobasic Acid.*		
4 HF+5 HCl	at 19° C.	162,078
4 ,, +4 ,, *	,,	151,760
4 ,, +3 ,, ...	,,	170,678
11.—*Monobasic Acid with Bibasic Acid.*		
2 H^2CrO^4+5 HCl	at 17° C.	72,445
2 ,, +4 ,, *	,,	62,056
2 ,, +3 ,, ...	,,	68,746
12.—*Monobasic Acid with Tribasic Acid.*		
Citric Acid+4 HF	at 20° C.	107,223
,, +3 ,, *	,,	102,319
,, +2 ,, ...	,,	107,223
13.—*Monobasic Acid with Tetrabasic Acid.*		
$H^4P^2O^7$+5 HNO^3	at 20° C.	26,795
,, +4 ,, *	,,	23,731
,, +3 ,, ...	,,	25,969

CHEMICAL EQUIVALENTS.		AVERAGE ENERGY.
14.—*Bibasic Acid with Bibasic Acid.*		
3 Oxalic Acid + 4 H^2SO^4	at 20° C.	81,232
4 ,, + 4 ,, *	,,	63,851
5 ,, + 4 ,, ...	,,	78,304
15.—*Bibasic Acid with Tribasic Acid.*		
2 Citric Acid + 4 H^2CrO^4	at 18° C.	924,835
2 ,, + 3 ,, *	,,	779,734
2 ,, + 2 ,, ...	,,	927,682
16.—*Bibasic Acid with Tetrabasic Acid.*		
2 $H^4P^2O^7$ + 5 H^2SO^4	at 20° C.	25,587
2 ,, + 4 ,, *	,,	21,648
2 ,, + 3 ,, ...	,,	23,542
17.—*Tribasic Acid with Tribasic Acid.*		
5 Citric Acid + 4 H^3PO^4	at 25° C.	3,900
4 ,, + 4 ,, *	,,	3,495
3 ,, + 4 ,, ...	,,	3,823
18.—*Tribasic Acid with Tetrabasic Acid.*		
3 $H^4P^2O^7$ + 5 Citric Acid	at 20° C.	13,119
3 ,, + 4 ,, *	,,	11,637
3 ,, + 3 ,, ...	,,	13,231
19.—*Monobasic Acid with Monobasic Salt.*		
4 KCl + 5 HF	at 17° C.	12,241
4 ,, + 4 ,, *	,,	11,063
4 ,, + 3 ,, ...	,,	12,241
20.—*Monobasic Acid with Bibasic Salt.*		
2 Na^2SO^4 + 5 HNO^3	at 18° C.	86,377
2 ,, + 4 ,, *	,,	77,446
2 ,, + 3 ,, ...	,,	88,480

CHEMICAL EQUIVALENTS.	21.—*Monobasic Acid with Tribasic Salt.*	AVERAGE ENERGY.
Na^2HPO^4+4 HCl	... ... at 19° C.	78,754
,, +3 ,, *	.. .. ,,	70,959
,, +2 ,,	... ... ,,	81,805

22.—*Monobasic Acid with Tetrabasic Salt.*

$Na^4P^2O^7$+5 HCl	... ... at 18° C.	8,637
,, +4 ,, *	.. .. ,,	8,123
,, +3 ,,	... . ,,	8,880

23.—*Bibasic Acid with Monobasic Salt.*

5 KCl+2 H^2SO^4	... ... at 15° C.	10,765
4 ,, +2 ,, *	... ... ,,	9,867
3 ,, +2 ,,	... ... ,,	11,637

24.—*Bibasic Acid with Bibasic Salt.*

4 K^2SO^4+5 H^2CrO^4...	... ... at 18° C.	52,438
4 ,, +4 ,, *	... ... ,,	41,413
4 .. +3 ,,	... ... ,,	46,049

25.—*Bibasic Acid with Tribasic Salt.*

4 Na^2HPO^4+7 H^2SO^4	... ... at 21° C.	37,706
4 ,, +6 ,, *	... ... ,,	30,931
4 ,, +5 ,,	... ... ,,	33,215

26.—*Bibasic Acid with Tetrabasic Salt.*

2 $Na^4P^2O^7$+5 H^2SO^4	... ... at 21° C.	14,004
2 ,, +4 ,, *	... ... ,,	12,145
2 ,, +3 ,,	.. ... ,,	13,008

27.—*Tribasic Acid with Monobasic Salt.*

4 NaCl+Citric Acid	... ... at 18° C.	10,526
3 ,, + ,, *	... ... ,,	8,707
2 ,, + ,,	... ... ,,	8,912

CHEMICAL EQUIVALENTS.	28.—*Tribasic Acid with Bibasic Salt.*	AVERAGE ENERGY.
7 $BaBr^2$	+4 Citric Acid ... ... at 21° C.	2,172
6 ,,	+4 ,, * ... ... ,,	2,044
5 ,,	+4 ,, ... ,,	2,324

29.—*Tribasic Acid with Tribasic Salt.*

3 Na^2HPO^4	+4 Citric Acid ... ... at 19° C.	4,330
4 ,,	+4 ,, *... ... ,,	3,900
5 ,,	+4 ,, ... ,,	4,461

30.—*Tribasic Acid with Tetrabasic Salt.*

3 (KCr2SO^4)	+5 Citric Acid ... at 23° C.	25,969
3 ,,	+4 ,, * ... ,,	24,035
3 ,,	+3 ,, ... ,,	25,559

31.—*Tetrabasic Acid with Monobasic Salt.*

5 KNO^3	+ $H^4P^2O^7$ ... ... at 28° C.	2,726
4 ,,	+ ,, * ... ... ,,	2,253
3 ,,	+ ,, ... ,,	2,659

32.—*Tetrabasic Acid with Bibasic Salt.*

5 Na^2SO^4	+2 $H^4P^2O^7$ ... ... at 25° C.	1,755
4 ,,	+2 ,, * ... ... ,,	1,577
3 ,,	+2 ,, ... ,,	1,841

33.—*Tetrabasic Acid with Tribasic Salt.*

5 Na^2HPO^4	+3 $H^4P^2O^7$ ... ... at 21° C.	936
4 ,,	+3 ,, * ... ... ,,	842
3 ,,	+3 ,, ... ,,	962

34.—*Tetrabasic Acid with Tetrabasic Salt.*

5 $Na^4P^2O^7$	+4 $H^4P^2O^7$ ... ... at 21° C.	171
4 ,,	+4 ,, * ... ... ,,	136
3 ,,	+4 ,, ... ,,	165

CHEMICAL EQUIVALENTS.	35.—*Monobasic Salt with Monobasic Salt.*	AVERAGE ENERGY.
4 KNO^3+5 KCl	... ... at 15° C.	275
4 ,, +4 ,, *	... ... ,,	211
4 ,, +3 ,,	... ... ,,	259
	36.—*Monobasic Salt with Bibasic Salt.*	
2 ($Ba2NO^3$)+5 NaCl	... ... at 15° C.	61·4
2 ,, +4 ,, *	... ... ,,	54·3
2 ,, +3 ,,	... ... ,,	61·4
	37.—*Monobasic Salt with Tribasic Salt.*—[Note.]	
2 LCl+Na^2HPO^4	... . at 13° C.	−1,390
3 ,, + ,, *	.. .. ,,	−1,558
4 ,, + ,,	... ... ,,	−1,295
	38.—*Monobasic Salt with Tetrabasic Salt.*	
$Na^4P^2O^7$+5 KCl	... ... at 16° C.	−7,657
,, +4 ,, *	.. .. ,,	−9,623
,, +3 ,,	... ... ,,	−7,951
	39.—*Bibasic Salt with Bibasic Salt.*	
4 Na^2CO^3+5 $K^2Cr^2O^7$	... ... at 17° C.	−23,785
4 ,, +4 ,, *	... ... ,,	−28,574
4 ,, +3 ,,	... ... ,,	−26,615
	40.—*Bibasic Salt with Tribasic Salt.*	
4 Na^2SO^4+3 Na^2HPO^4	... ... at 20° C.	779
3 ,, +2 ,, *	... ... ,,	727
2 ,, +3 ,,	... .. ,,	818
	41.—*Bibasic Salt with Tetrabasic Salt.*	
5 Am^2SO^4+2 $Na^4P^2O^7$	... ... at 25° C.	−1,134
4 ,, +2 ,, *	... ... ,,	−1,346
3 ,, +2 ,,	... ... ,,	−1,163

Note.—The loss of voltaic energy in this case and several others is indicated by a maximum minus number, instead of a minimum plus one.

CHEMICAL EQUIVALENTS.	42.—*Tribasic Salt with Tribasic Salt.*	AVERAGE ENERGY.
4 Na Citrate + 5 Na^2HPO^4	at 22° C.	381
4 ,, + 4 ,, *... ...	,,	365
4 ,, + 3 ,, ...	,,	400

43.—*Tribasic Salt with Tetrabasic Salt.*

5 Na^2HPO^4 + 3 $Na^4P^2O^7$	at 21° C.	−1,715
4 ,, + 3 ,, *	,,	−1,894
3 ,, + 3 ,, ...	,,	−1,723

44.—*Tetrabasic Salt with Tetrabasic Salt.*

5 $Am^4P^2O^7$ + 4 $Na^4P^2O^7$	at 23° C.	−351
4 ,, + 4 ,, *	,,	−379
3 ,, + 4 ,, ...	,,	−337

45.—*Sugar + Common Salt.*

2 Cane Sugar + 5 NaCl	at 11° C.	−43·6
2 ,, + 4 ,, *	,,	−47
2 ,, + 3 ,, ...	,,	−44·5

It is evident in all these cases, from the proportions of substances required to reduce the energy to the smallest number, that the two uniting substances must be chemically equivalent to each other.

The foregoing series of examples show that all kinds of soluble elements, acids, acid, neutral and alkaline salts, etc., whilst in aqueous solution together, unite with each other indiscriminately in the definite proportions by weight of their ordinary chemical equivalents.

As in every one of these cases, either one of the two ingredients may be replaced by almost any other soluble ingredient of equal chemical value, and as each of these instances is only a solitary example of a large number of the same class of compounds, and each compound is capable of uniting with nearly every other compound individually whilst in solution, and these more complex ones are similarly capable

of uniting with others of equal chemical value almost without limit, except in cases where separation by precipitation or evolution of gas occurs (see Roy. Soc. Proc., Vol. XLV., p. 266; also "Loss of Voltaic Energy by Chemical Union," Proc. Birm. Phil. Soc., 1888, Vol. VI., part 2), it is evident that the total number of possible "solution-compounds" is exceedingly great—certainly thousands.

The fact, which has been so largely disclosed by means of the voltaic balance, that *all kinds of chemical substances, when in mutual solution, unite with each other indiscriminately and irrespective of the chemical nature of the substances*, has an important bearing upon chemical action, and throws great light upon the cause of chemical union.

The following are the average amounts of available voltaic energy of the ingredients employed in making the above compounds, and from these numbers and the foregoing ones the amounts of loss of such energy which occurred during the formation of the compounds can be ascertained:—

	CENTIGRADE.	AVERAGE VOLTAIC ENERGY.
Cl	at 11°	1,282,000,000
Br	13·7°	81,022,500
HCl	16·5°	9,344,092
H^2SO^4	19°	3,900,941
I	13·5°	3,310,985
HNO^3	19°	3,204,395
H^2CrO^4	18°	1,611,111
HF	18°	1,151,363
KCl	8°	699,803
NaCl	15°	207,589
Citric Acid	22°	36,597
KCr $2SO^4$	19°	35,902
H^3PO^4	26°	34,952
$H^4P^2O^7$	17°	20.336
AmCl	13·5°	15.069

	CENTIGRADE.	AVERAGE VOLTAIC ENERGY.
KNO^3	20°	10,841
Oxalic Acid	15°	9,450
$K^2Cr^2O^7$	13°	7,156
Am^2SO^4	20°	2,634
K^2SO^4	12°	2,274
Na^2HPO^4	15°	2,057
Na^2SO^4	13°	2,020
Na Citrate	22°	1,466
$Am^4P^2O^7$ (variable)	23°	327
LCl	16°	204
Water	16°	0
Cane Sugar	15°	– 67·8
Ba $2NO^3$	19°	– 163
$BaBr^2$	17°	– 163
$Na^4P^2O^7$	18°	– 232
Na^2CO^3	19°	– 14,795

The amounts of voltaic energy with zinc-platinum couples of more than three hundred "solution-compounds" are given in a paper on "Relative Amounts of Available Voltaic Energy of Aqueous Solutions" (Ibid., 1889, Vol. VII., part 1).

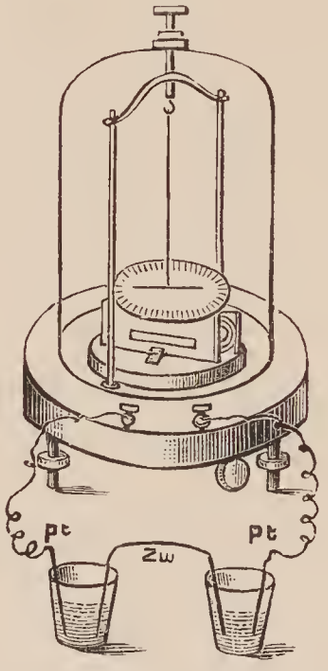
IV.—*Relative Amounts of available Voltaic Energy of Aqueous Solutions.*

By G. Gore, LL.D., F.R.S.

In the present research the amounts of voltaic energy were those excited by means of a zinc and platinum couple in solutions of the substances in distilled water, the electro-motive force of the couple in the water alone, before addition of the substance, being equal to ·98 volt, and balanced either by a suitable thermo-electric pile (see Proc. Birm. Philos. Soc., Vol. IV., p. 130, or "The Electrician," Vol. XII., p. 414), or, much more conveniently, by an exactly similar couple in distilled water, and the substance then added in minute quantities at a time to the water of the testing cell until the needles of an ordinary torsion astatic galvanometer of 100 ohms resistance, included in the circuit, visibly commenced to move, the current from the testing cell being usually the strongest. The minimum amount of electro-motive force required to move the needles was about ·00013 volt. For greater convenience in working, the two pairs of metals were attached to a frame and rack work, so as to slide up or

down, and be supported either in or out of the liquids (see "The Electrician," 1889, Vol. XXIII., p. 171). N.B.—The measurements do not include the amounts of voltaic energy lost by "local action."

Each amount of energy given represents the proportion of water required to dilute one part by weight of the substance



in order to make the effect upon the needles sufficiently feeble; it is also the average of two numbers, *viz.*: one of the proportion of substance which was barely strong enough to visibly move the needles, and another of the proportion which just visibly produced that effect; the needles were observed through a magnifying glass. As the amount of mechanical work done by the current upon the needles at the point of measurement was the same in all the cases, and as all the essential conditions of action were unchanged, except the chemical composition of the liquid in the testing cell, the numbers given represent approximately the relative amounts of available voltaic energy exerted between the zinc and the respective substances in solution at the given temperatures. Polarisation was sufficiently excluded by heating the platinum to redness when necessary; the solutions used, however, were usually so extremely weak, and, by much experience in working, the point

of measurement could be so quickly and nearly arrived at that very little polarisation occurred.

As in some cases the numbers obtained varied greatly with small changes of chemical composition of the substance, they are, of course, subject to the influence of any impurity present; this influence has been separately examined. The substances employed were very generally of a high degree of purity, and any that were suspected to be impure were analysed; a large number of them had been analysed and purified from time to time. Distilled water was used in making all the solutions. In the cases where minus numbers are given the electro-motive force and voltaic energy of the testing cell was *less* than that of the opposing one excited by water alone.

In order to detect the influence of any invisible film of oxide, &c., formed upon the surface of the zinc in the active cell, in every instance after the determination, the active cell was emptied of solution, refilled with distilled water, and the couple immersed in the water to ascertain whether it exactly balanced the couple in the idle cell. If the two were equal, it was concluded that no interposing film had formed upon the zinc. The fact of the method being nearly a null one reduced largely the chances of polarisation.

It is evident that in such experiments only rarely, and by what is termed "accident," are the two opposing electro-motive forces perfectly equal, the particular number of pairs of the thermo-electric pile is either a little too strong or a little too weak to exactly balance the cell; or, if two cells are employed. they are never exactly alike in potential. Further, as it is highly probable that even the very smallest proportion of an acid, &c. (insufficient to move the needle), when added to the water, begins to alter the potential (see "Effects of Different Positive Metals, &c.," Roy. Soc. Proc., Vol. XLIV., p. 368); then, with all proportions of substance below that which visibly moves the needles, the two opposing forces appear to be balanced when they are not so. Also the further addition of acid, &c.,

not only alters the potential, but diminishes the total resistance until sufficient current passes to produce a visible movement of the needles.

The question whether the two forces were sufficiently nearly balanced when they shewed no effect upon the needles was tested by immersing the usual zinc wire and platinum sheet couple in distilled water, balancing its potential through the usual galvanometer by means of that of a small Daniell's cell plus a sufficient portion of the thermo pile, and allowing the arrangement to remain undisturbed during fifteen minutes to develop polarisation, and visibly change the balance; no such effect took place. In another trial the same couple was immersed in a solution of four grains of potassic chlorate and 465 grains of water; then, carefully balanced by the thermo pile and allowed to remain fifteen minutes, the balance was again not visibly altered. The extreme sensitiveness of the method is a further guarantee that the two forces were sufficiently nearly balanced when they showed no effect upon the needles.

In order to test whether the change of resistance, due to addition of acid, &c., to the couple in distilled water below the "minimum point" of change, had any effect upon the "minimum proportion" of the substance required to visibly alter the balance, and as one of the sources of resistance was the limited surfaces of the metals, I took plates of the two metals, each 3·5 centimetres wide, and immersed them 5·0 centimetres deep, and 1·0 centimetre apart in the water, and then balanced the potential of this cell through the ordinary galvanometer by that of the usual one with the smaller pieces of metal. On then adding chlorine to the water of the larger couple, the "minimum proportion" was found to lie between one part in 1,110,660,000 and 1,300,000,000, the same as with the ordinary couple. By other special trials of the same kind with hydrochloric acid and with potassium chlorate, the minimum proportions of substance required were found to be the same as when the usual sized pieces of metal were employed. From these results I conclude

that the change of resistance due to change of size of metal had no conspicuous effect.

In each of the instances of measurement of energy of complex substances, the amount of energy given is that of the true or definite "solution compound," not containing any excess of either ingredient. In order to ascertain the real combining proportion and formula of each complex substance, additional measurements were in every instance made, both of the amount of energy of the compound with an excess of one of its ingredients, and with an excess of the other (see Roy. Soc. Proc., Vol. XLV., p. 265; also Proc. Birm. Philos. Soc., Vol. VI., Part 2; *Ibid.*, Vol. VII., Part 1). These extra measurements are omitted from the table. The true "solution compound" was in every instance indicated by a depression of voltaic energy, and the amount of energy in every case increased distinctly on adding a limited excess of either ingredient. The combining proportion indicated by the balance was in every case the same as the ordinary chemical equivalent; this will be at once recognised by the formulæ given. The list includes about 324 of such compounds.

Of the various solutions the following are the most conspicuously isomeric ones:—19 and 93—42, 58—53, 66—60, 104—73, 94—77, 129—92, 103—98, 191—110, 165—113, 136—118, 123—125, 173—128, 172—131, 190—147, 198—152, 226—168, 213—184, 238—187, 212—200, 208—203, 207—221, 223—237, 255—299, 306—317, 361—372, 382—373, 375 and 377, 379.

In the case of isomeric solutions, caution must be exercised in accepting the numbers of one of each couple, the one having the largest amount of energy, because that one is usually unstable, liable to decompose, especially by elevation of temperature, and become converted into its isomer, with simultaneous loss of voltaic energy. KI+Cl (voltaic energy= 2,045,138), and KCl+I (=64,536) constitute an example (see Roy. Soc. Proc. Vol. XLV., p. 440). $Na^2SO^4+2HNO^3$ and $2\,NaNO^3+H^2SO^4$ is another (see Phil. Mag., Oct., 1889, p. 290).

The plus sign in the formulæ is used to separate the equivalent quantities. The amounts of voltaic energy are relative, not actual.

Amounts of Voltaic Energy with a Zinc and Platinum Couple.

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
1.	Chlorine	at 11	1,282,000,000
2.	Bromine	13·7	81,022,500
3.	HClO	16	19,635,286
4.	I+Cl	20	11,686,507
5.	HCl	16·5	9,344,092
6.	2 HCl+H^2O^2	20	9,291,244
7.	$HClO^4$	16	6,628,450
8.	H^4CrO^4+2 Cl	21	6,532,216
9.	$HClO^3$	12	4,473,136
10.	KF+Cl	18	4,090,277
11.	H^2SO^4	19	3,900,941
12.	H^2SO^4+2 HCl	14	3,804,909
13.	Iodine	13·5	3,310,985
14.	HNO^3	19	3,204,395
15.	2 HNO^3+H^2SO^4	11	3,150,000
16.	HCl+HClO	16	3,087,001
17.	HNO^3+HCl	18	2,435,240
18.	KCl+Cl	20	2,331,005
19.	KBr+Cl	20	2,287,326
20.	KI+Cl	17	2,045,138
21.	HCl+Br	10	1,992,416
22.	HBr+Cl	10	1,971,217
23.	Laevotartaric acid+2 Cl ...	19	1,907,662
24.	Br+Cl	20	1,817,901
25.	Racemic acid+2 Cl ...	21	1,708,454
26.	HBr	10	1,636,111
27.	H^2CrO^4	18	1,611,111
28.	Acetic acid	13	1.585,760
29.	$KClO^3$+Cl	20	1,501,907

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
30.	Na^2CO^3, Na^2SO^3+4 Cl ...	23	1,466,964
31.	H^2SO^3	15	1,328,779
32.	Succinic acid+2 Cl	21	1.319,625
33.	AmCl+Cl	12	1,181,545
34.	HF	18	1.151,363
35.	H^2SO^4 +2 Br	11	1,134,342
36.	Citric acid+3 Cl	21	1,065.693
37.	Oxalic acid+2 Br	24	1.050,367
38.	H^2CrO^4+ 2 Br	18	1,050,367
39.	2 HNO^3, H^2SO^4+4 HCl ...	24	980,739
40.	HClO, HCl+2 $HClO^3$ ...	21	963,955
41.	Na^2CO^3+ 2 Cl	19	945,907
42.	HIO^3+Cl	27	887,571
43.	KNO^3 + Cl	17	863,776
44.	$KMnO^4$	18	845,402
45.	HNO^3, HCl+H^2SO^4	24	831.254
46.	2 Citric acid+3 H^2CrO^4 ...	18	799,734
47.	4 HNO^3+H^2SO^4, 2 HCl ...	24	775,200
48.	HCl+Cl	21	739,772
49.	HCl+I	19	739,772
50.	KCl	8	699,803
51.	H^2SO^4+2 Cl	15	678,697
52.	NaBr+Br	15	668,601
53.	AmBr+Cl	10	654,444
54.	Citric acid+3 Br	20	636,956
55.	HNO^3+I	19	612,916
56.	$H^4P^2O^7$+4 Br	20	586.785
57.	HBO^2+Cl	18	582,540
58.	I+$HClO^3$	25	567,177
59.	Formic acid (H^2CO^2) ...	19	545.535
60.	$BaCl^2$+2 Br	20	486,430
61.	HNO^3+Cl	19	472,701
62.	NaCl+Cl	15	467,460
63.	HBO^2+HCl	15	446.363
64.	Racemic acid+2 Br ...	20	428,623

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
65.	I + Br	20	337,627
66.	AmCl + Br	14	327,222
67.	KF + Br	18	323,478
68.	KCl + HCl	22	320,952
69.	HBO^2 + I	18	315,000
70.	AmBr + Br	13	295,514
71.	AmCl, Br + AmBr, Cl ...	12	293,270
72.	Oxalic acid + 2 Cl	21	288,400
73.	NaBr + Cl	19	288,000
74.	2 I + H^2SO^4	13	286,832
75.	Citric acid + 3 I	20	275,392
76.	H^2CrO^4 + 2 I	20	272,685
77.	NaI + Cl	19	272,686
78.	RbI + Cl	8	233,729
79.	RbCl + I	8	233,729
80.	$KClO^3$ + Br	14	232,378
81.	$H^4P^2O^7$ + 2 H^2SO^4	20	216,648
82.	NaCl	15	207,589
83.	2 HF + H^2SO^4	15	206,030
84.	NaCl, KCl + 2 Cl	16	204,059
85.	KIO^3 + Cl	18	203,317
86.	Succinic acid + 2 Br ...	17	192,611
87.	HIO^3 + I	18	191,826
88.	Racemic acid + 2 I	20	184,943
89.	NaHO + HCl (Neutral) ...	16	170,428
90.	KNO^3 + Br	19	167,362
91.	NaBr, I + NaCl, Cl	24	163,611
92.	LBr + Cl	19	161,690
93.	KCl + Br	16	160,833
94.	NaCl + Br	19	152,499
95.	KF + HCl	19	151,760
96.	Am^2SO^4 + Succinic acid ...	23	144,264
97.	Oxalic acid + 2 I	22	136,343
98.	NaF + HCl	14	132,170
99.	HF + HNO^3	19	129,130

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
100.	Sulphanilic acid	21	121,990
101.	Oxalic acid + 2 HF	20	119,578
102.	$HBO^2 + HF$	17	119,578
103.	LCl + Br	14	118,780
104.	$BaBr^2 + 2$ Cl	17	111,794
105.	$Na^4P^2O^7 + 4$ I	16	109,940
106.	KBr + Br	16	109,502
107.	$2 HBO^2 + H^2SO^4$	20	109,166
108.	$H^2CrO^4 + 2$ HBr	21	107,791
109.	$K^2CrO^4 + 2$ Cl	18	105,710
110.	$2 KNO^3 + H^2SO^4$	18	105,357
111.	Citric acid + 3 HF	20	102,319
112.	Oxalic acid + H^2CrO^4 ...	18	96,241
113.	KI + Br	18	90,128
114.	NaI + I	16	89,750
115.	$K^2CO^3 + 2$ Cl	16	82,680
116.	$K^2CrO^4 + 2$ Br	17	82,680
117.	$Na^4P^2O^7 + 4$ Br	21	82,289
118.	$2 NaCl + H^2SO^4$	16	77,909
119.	$Na^2SO^4 + 2 HNO^3$	18	77,446
120.	$H^2CrO^4 + H^2SO^4$	13	74,368
121.	Oxalic Acid + 2 HCl ...	20	73,050
122.	$Na^2HPO^4 + 3$ HCl	19	70,959
123.	$Na^2SO^4 + 2$ HCl	16	68,346
124.	KBr	9	66,909
125.	$L^2SO^4 + 2$ HCl	18	65,444
126.	$KIO^3 + Br$	11	65,444
127.	KCl + I	13	64,536
128.	NaI + Br	16	64,354
129.	NaCl + I	20	62,836
130.	$H^2CrO^4 + 2$ HCl	17	62,056
131.	$KIO^3 + HBr$	17	62,056
132.	Succinic acid + H^2SO^4 ...	11	59,893
133.	KF + I	13	59,560
134.	Tartaric acid + H^2SO^4 ...	10	56,717

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
135.	$KCr2\,SO^4+4\,NaCl$ ...	25	54,928
136.	$KBr+I$	13	54,826
137.	Potash alum	15·5	54.536
138.	Oxalic acid $+H^2SO^4$	16	54,536
139.	Citric acid $+3$ HCl	22	54,536
140.	NaI, $I+KCl$, Br	20	53,508
141.	Oxalic acid $+2\,HNO^3$ ...	20	52,700
142.	$KBrO^3+I$	20	52,615
143.	Succinic acid $+2$ I	18	51,130
144.	Racemic acid $+H^2SO^4$ ...	13	51,128
145.	K^2SO^4+2 Cl	20	50,648
146.	$2\,HF+H^2CrO^4$	16	49,333
147.	KNO^3+HCl	20	49,333
148.	$KClO^3+HCl$	16	47,142
149.	Na^2CO^3+2 I	20	45,988
150.	KNO^3+I	20	45.746
151.	$H^2CrO^4+2\,HNO^3$	20	42,412
152.	$K^2SO^4+H^2CrO^4$	18	41.413
153.	K^2SO^4+2 I	20	38,900
154.	$KI+I$	18	36,597
155.	Citric acid	22	36,597
156.	Chrome alum	19	35,902
157.	H^3PO^4	26	34,952
158.	Na^2CO^3+2 Br	20	32,722
159.	$2\,NaNO^3+H^2SO^4$	18	32,722
160.	K^2CO^3+2 Br	15	32,046
161.	$NaCl+HCl$	17·5	31,083
162.	$2\,Na^2HPO^4+3\,H^2SO^4$ ...	21	30,931
163.	$Na^2SO^4+H^2SO^4$	12	30,768
164.	Succinic acid...	19	29,906
165.	$K^2SO^4+2\,HNO^3$	17·5	29,590
166.	2 Citric acid $+3\,H^2SO^4$ ...	16	29,590
167.	Citric acid $+3\,HNO^3$ ...	20	29,651
168.	$2\,KIO^3+H^2SO^4$	14	29,519
169.	$LF+HCl$	18	26,560

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
170.	$2\,Na^2HPO^4+6\,I$	21	26,556
171.	$LCl+HF$	17	26,388
172.	$NaBr+I$	14	25,709
173.	$2\,LCl+H^2SO^4$	18	25,566
174.	$K^2SO^4+2\,Br$	19	25,214
175.	$K^2SO^4+2\,HNO^3$	18	25,205
176.	$K^2CO^3+2\,I$	13	24,104
177.	3 Cr alum + 4 Citric acid	23	24,035
178.	$H^4P^2O^7+4HNO^3$	20	23,731
179.	$NaNO^3+HNO^3$	12	23,371
180.	$Na^2S^2O^3+2I$	18	23,024
181.	$H^4P^2O^7$	20	22,723
182.	$H^4P^2O^7+2H^2SO^4$	20	21,648
183.	K^2CrO^4+2I	17	21,071
184.	$NaCl+HNO^3$	11	21,070
185.	$Sr2NO^3+2HCl$	20	20,850
186.	$ZnCl^2$	17	20,336
187.	$K^2SO^4+2\,HCl$	15	20.125
188.	$KHSO^4$	16	18,298
189.	$K^2SO^4+H^2SO^4$	17	18,016
190.	HIO^3+KBr	18	17,042
191.	$NaCl+HF$	16·5	16,700
192.	KI	19	16,361
193.	$MgCl^2$ (Alkaline)	17	16,361
194.	Succinic acid $+H^2CrO^4$	20	16,361
195.	$SrCl^2+2\,HNO^3$	20	16,233
196.	Ammonia alum	18	15,857
197.	AmCl	13·5	15,069
198.	$KCl+HNO^3$	17	15,018
199.	KIO^3+I	21	14,004
200.	$ZnCl^2+H^2SO^4$	15	13,755
201.	$ZnSO^4$	22	13,654
202.	$2\,AmCl+MgCl^2$	20	13,070
203.	$KF+HCl$	20	12,902
204.	$2\,NaCl+KCl, HCl$	15	12,529

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
205.	$Na^4P^2O^7+2\ H^2SO^4$	21	12.145
206.	3 $H^4P^2O^7+4$ Citric acid ...	20	11,637
207.	KCl+HF	17	11,063
208.	$ZnSO^4+2$ HCl	18	11,063
209.	HIO^3	18	10,950
210.	KNO^3	20	10,841
211.	3 NaBr+Citric acid ...	14	10,129
212.	2 KCl+H^2SO^4	15	9,867
213.	$K^2SO^4+2\ HIO^3$	19	9,745
214.	3 Succinic acid+2 Citric acid	18	9,623
215.	HBO^2+HNO^3	18	9,558
216.	Oxalic acid	15	9,450
217.	3 Oxalic acid+2 Citric acid	18	9,436
218.	Laevotartaric acid	17	9,308
219.	Citric acid+3 NaCl ...	18	8,707
220.	3 Tartaric acid+2 Citric acid	18	8,637
221.	Sr 2 NO^3+2 HCl	13	8,637
222.	$Na^4P^2O^7+4$ HCl	18	8,123
223.	$SrCl^2+2\ HNO^3$	13	8,094
224.	3 Racemic acid+2 Citric acid	19	7,700
225.	KI+KCl	10	7,571
226.	$K^2CrO^4+H^2SO^4$	18	7,509
227.	$K^2Cr^2O^7$	13	7,156
228.	Ba 2 NO^3+2 HCl	20	7,113
229.	Racemic acid	20	7,069
230.	2 $HBO^2+H^2CrO^4$	17	6,988
231.	$BaCl^2+2\ HNO^3$	13	6,929
232.	$K^2CrO^4, H^2CrO^4+K^2SO^4, H^2SO^4$	18	6,790
233.	$K^2CrO^4+H^2CrO^4$	16	6,396
234.	$K^2CrO^4, H^2SO^4, +K^2SO^4, H^2CrO^4$	15	6,318
235.	Dextrotartaric acid	15	6,099
236.	NaCl+KCl	20	5,959
237.	KIO^3+HCl	17·5	5,350
238.	$NaNO^3$+HCl	12	4,959
239.	3 HBO^2+Citric acid ...	18	4,675

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
240.	K^2SO^4+2 HIO^3	19	4,683
241.	Na^2HPO^4+Citric acid ...	19	3,900
242.	Citric acid+H^3PO^4	25	3,495
243.	Laevotartaric acid+Oxalic acid	22	3,405
244.	2 HBO^2+Oxalic acid ...	18	3,228
245.	$KClO^3$+I	19	2,737
246.	Am^2SO^4	20	2,634
247.	Salicylic acid	15	2.591
248.	2 $NaNO^3$,H^2SO^4+Na^2SO^4,2 HNO^3	13	2,410
249.	K^2SO^4...	12	2,274
250.	4 KNO^3+$H^4P^2O^7$	28	2,253
251.	Na^2HPO^4	15	2,057
252.	3 $BaBr^2$+2 Citric acid ...	21	2,044
253.	Na^2SO^4	13	2,020
254.	Racemic acid+$K^2Cr^2O^7$ ...	20	1,940
255.	HIO^3+KCl	19	1,817
256.	NaBr	18	1,636
257.	NaI	17	1,636
258.	2 Na^2SO^4+$H^4P^2O^7$ ...	25	1,577
259.	NaCitrate	22	1,466
260.	KF	18	1,278
261.	Na^2SO^4+2 KBr	21	1,264
262.	2 NaBr+K^2SO^4	21	1,086
263.	Beryllium sulphate ...	21	993
264.	NaBr+KI	15	938
265.	NaI+NaBr	15	931
266.	NaI+KBr	17	917
267.	4 Na^2HPO^4+3 $H^4P^2O^7$ ...	21	842
268.	KBr+KCl	17	786
269.	KCr Oxalate (anhydrous) ...	16	777
270.	$MgCl^2$+KI	20	727
271.	3 Na^2SO^4+2 Na^2HPO^4 ...	20	727
272.	2 LCl+K^2SO^4	18	674
273.	L^2SO^4+2 KCl	18	671
274.	Na^2HPO^4+3 NaCl ...	13	605

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY
275.	AmBr	15	545
276.	KBr+NaCl	11	539
277.	KIO^8	15	463
278.	$BaCl^2$	15	457
279.	NaAmTartrate	18	426
280.	NaTartrate	18	399
281.	HBO^2(traces of H^2SO^4) ...	20	393
282.	$SrCl^2$	15	365
283.	NaCitrate+Na^2HPO^4 ...	22	365
284.	$KBrO^3$	15	364
285.	$AmNO^3$(neutral)	15	357
286.	Chloride of Ethylamine ...	15	334
287.	$Am^4P^2O^7$ (variable)	23	327
288.	Rochelle salt	15	298
289.	$KClO^3$	10	240
290.	AmTartrate	16	227
291.	$MgSO^4$	20	216
292.	KNO^3+KCl	15	211
293.	LCl	16	204
294.	L^2SO^4	17·5	201
295.	$CaCl^2$	16	181
296.	$KClO^4$	12	163
297.	$NaNO^3$	19	163
298.	Sr2 NO^3	12	163
299.	KI+NaCl	13	150
300.	Na^2SO^4+Am^2SO^4	19	140·5
301.	Asparagine	19	140
302.	$NaNO^3$+KI	14	136
303.	$Na^4P^2O^7$+$H^4P^2O^7$	21	136
304.	NaI+KNO^3	12	135
305.	K^2SO^4+Am^2SO^4	20	127·5
306.	NaI+KCl	15	121
307.	LCl+KCl	17	116
308.	$SrCl^2$+$BaCl^2$	15	116
309.	Na^2SO^4+K^2SO^4	17	112·5

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
310.	NaSalicylate	19	108·5
311.	LCl + NaCl	15	105·5
312.	LCl + AmCl	15	102
313.	$SrCl^2 + 2\,KCl$	17	101·5
314.	$MgSO^4 + ZnSO^4$	20	99
315.	BaO, H^2O	17	98
316.	RbCl	17	97
317.	$Na^2SO^4 + 2\,KCl$	12	97
318.	$Na^2SO^4 + ZnSO^4$	20	97
319.	AmCl + KCl	16	94·5
320.	$2\,LCl + SrCl^2$	20	92
321.	$Am^2SO^4 + ZnSO^4$	19	92
322.	$MgSO^4 + Na^2SO^4$	20	92
323.	$K^2SO^4 + ZnSO^4$	18	90·5
324.	$SrCl^2 + 2\,NaCl$	15	87·5
325.	$SrCl^2 + 2\,AmCl$	16	86
326.	$MgSO^4 + Am^2SO^4$	20	86
327.	$NaNO^3 + KNO^3$	20	85·5
328.	$MgSO^4 + K^2SO^4$	18	84·5
329.	$NaClO^3$	18	81·7
330.	AmCl + NaCl	16	81·5
331.	Am Oxalate	19	81·5
332.	$Sr\,2\,NO^3 + 2\,KCl$	18	76
333.	$SrCl^2 + 2\,KNO^3$	18	76
334.	$Ba\,2\,NO^3 + 2\,KNO^3$	19	75
335.	$2\,NaNO^3 + Na^2SO^4$	12	74·7
336.	$AmNO^3 + KNO^3$	20	74
337.	$2\,LCl + BaCl^2$	18	74
338.	$Ba\,2\,NO^3 + 2\,AmNO^3$ ...	19	72
339.	$K^2SO^4, 2\,NaCl + 4\,AmCl$ ...	15	69
340.	$BaCl^2 + 2\,KCl$	17	68
341.	$BaCl^2 + 2\,NaCl$	15	66
342.	$Sr\,2\,NO^3 + 2\,NaCl$	17	65
343.	$2\,NaNO^3 + SrCl^2$	17	62·8
344.	$L^2SO^4 + 2\,NaCl$	14	62

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
345.	$2\,LCl + Na^2SO^4$	14	59·5
346.	$BaCl^2 + 2\,AmCl$	16	56·5
347.	$2\,NaNO^3 + BaCl^2$	15	54·3
348.	$KClO^3 + KCl$	20	53
349.	$K^2SO^4 + KCl$, NaCl	19	53
350.	$KClO^3 + NaCl$	13	52·5
351.	$K^2SO^4 + 2\,KNO^3$	18	52
352.	$Ba\,2\,NO^3 + 2\,NaCl$	13	50·5
353.	$NaNO^3 + AmNO^3$	20	49
354.	$K^2CrO^4 + KCl$	15	42·5
355.	$CaCl^2$ (in 7 vols. of glycerine and 3 of H^2O)	18	41·9
356.	Na^2SO^4, $K^2SO^4 + 2\,NaCl$, 2 KCl	16	41
357.	$HBO^2 + KCl$	12	35·3
358.	$Ba2\,NO^3 + 2\,NaNO^3$	19	31
359.	$KClO^3$, KCl + Am oxalate ...	11	28·3
360.	$K^2SO + 2\,KCl$	21	26·5
361.	$K^2SO^4 + 2\,NaCl$	21	26·5
362.	$CaCl^2$ (in nearly anhydrous alcohol)	20	23·4
363.	K^2SO^4, 2 NaCl, 4 AmCl, 8 KCl $+16\,KNO^3$	15	19
364.	K^2CrO^4	13	6·17
365.	Water...	16	0
366.	Alcohol (anhydrous)... ...	15	−6·65
367.	Urea	19	13·6
368.	$Sr2\,NO^3 + 2\,NaNO^3$	18	20·4
369.	$SrCl^2 + 2\,KBrO^3$	16	29·9
370.	$Rb^2Cr^2O^7$	15	45·5
371.	Cane sugar + 2 NaCl ...	11	47
372.	$NaCl + KNO^3$	17	48·5
373.	$BaCl^2 + 2\,KNO^3$	16	62·5
374.	Cane sugar	15	67·8
375.	$Ba2\,NO^3 + 2\,KCl$	15	73·5
376.	$Na^2S^2O^3$	22	79

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
377.	$KCl + AmNO^3$	10	−88·0
378.	Carbolic acid (solid and colourless)	19	90·5
379.	$AmCl + KNO^3$	10	95·1
380.	$NaClO^3 + KNO^3$	26	104·3
381.	$NaNO^3 + KClO^3$	26	107
382.	$NaNO^3 + KCl$	16	108
383.	$NaNO^3 + KBr$	25	113·3
384.	$NaBr + KNO^3$	25	113·3
385.	KNO^2	13	120
386.	Na^2SO^3 (traces of Na^2SO^4) ...	22	133
387.	$KNO^2 + KNO^3$	13	144
388.	Am Diphosphate	18	158
389.	$BaBr^2$	17	163
390.	Ba2 NO^3	19	163
391.	$Na^2SO^3 + Na^2S^2O^3$	17	169
392.	$Na^4P^2O^7$	18	232
393.	Aniline (colourless)	12	253
394.	$Am^4P^2O^7 + Na^4P^2O^7$	23	379
395.	Caffeine (perfectly white) ...	18	416
396.	Microcosmic salt (anhydrous)	15	467
397.	Am Vanadate	17	467
398.	$NaClO^3 + KBr$ (very variable)	23	478
399.	$KClO^3 + NaBr$ (very variable)	23	495
400.	$LBr + LCl$	16·5	605
401.	Aniline (dark brown) ...	18	816
402.	$Na^4P^2O^7 + 2\ Am^2SO^4$ ...	25	1,278
403.	$2\ Am^2SO^4 + Na^4P^2O^7$ ...	25	1,346
404.	$3\ LCl + Na^2HPO^4$	13	1,558
405.	LBr	13	1,574
406.	$3\ Na^4P^2O^7 + 4\ Na^2HPO^4$ ...	21	1,894
407.	Nicotine (brownish)	19	3,159
408.	Picric acid (variable) ...	17	3,585
409.	LF	18	3,635
410.	$Na^4P^2O^7 + 4\ KCl$	16	9,623

No.		TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
411.	2 NaCl, 2 KCl ; Na^2SO^4. K^2SO^4; 4 K^2CrO^4 + 8 K^2CO^3 ...	11	−11.435
412.	Na^2O, 2 B^2O^3	15	12,494
413.	Na^2CO^3	19	14,795
414.	Na^2CO^3 + $K^2Cr^2O^7$	17	28,574
415.	Rb^2CO^3	19	33,607
416.	$Na^4P^2O^7$ + 4 NaCl	19	38,023
417.	CaO	18	52,777
418.	K^2CO^3...	19	91,493
419.	Na^2CO^3, Na^2SO^3 + $Na^4P^2O^7$	17	97,426
420.	NaHO (" By alcohol ") ...	18	201.989
421.	2 NaHO + K^2CO^3	17	235,634
422.	Am bicarbonate (variable) ...	16	237,318
423.	Methylamine	19	241,019
424.	KHO (" By alcohol ") ...	17	270,985
425.	Am Sesquicarbonate (variable)	16	274,219
426.	H^3N (variable)	15	297,791
427.	Na^2CO^3 + Na^2SO^3	17	437,846
428.	H^3N + KHO	15	684,732
429.	2 (H^3N, KHO) + K^2CO^3, Na^2SO^3	16	959,817

The above order may be viewed as that of a volta tension series of liquids with a zinc platinum couple.

The influence of the kind of solvent upon the amount of voltaic energy is shown by the measurements made with chloride of calcium ; this substance when dissolved in distilled water at 16°C. gave 181 ; in 7 volumes of glycerine and 3 of water at 18°C., 41·9 ; and in nearly anhydrous alcohol at 20°C., 23·4.

A chemical analysis of the caustic soda employed, calculated for the anhydrous substance, gave 80·04 per cent. of NaHO and 19·96 of Na^2CO^3; and of the caustic potash, 77·6 KHO and 22·4 K^2CO^3. With a very pure sample, containing 97·64 per cent. KHO and 2·36 K^2CO^3, the amount of energy was −252,533.

A sample of potassic cyanide, containing 92·11 per cent. of KCy, gave −148,737.

It is interesting to notice that cane sugar acts as a dyad in uniting with common salt; the action, however, is not very distinct.

In this table the elementary halogens head the list; then follow the acids, acid salts, neutral salts, alkaline salts, and finally the alkalies, and complex compounds of alkaline reaction. Picric and carbolic acids are, however, amongst the substances yielding minus numbers. Strong solutions of carbolic acid, ammonia, or potassic cyanide (above 1 part KCy in 3,229 parts of water), when balanced by equal electro-motive force, gave plus numbers.

The great decrease of electro-negative energy attending increasing complexity of chemical composition of the compound is strikingly shown by some of the mixtures employed, and is usually greater the nearer the aggregate of molecules approaches that of a mechanical mixture, and suggests the question whether the dissolved alkalies are usually composed of more complex molecules than the dissolved acids, which are nearly all of them near the other end of the scale.

By arranging well-known chemical groups of substances in series, with their amounts of voltaic energy, the latter forms, in various cases, a regular gradation of values; for instance, chlorine with potassic fluoride, chloride, bromide, and iodide; bromine with those salts; iodine with potassic chloride, bromide, and iodide; chlorine with sodic chloride, bromide, and iodide; also either potassic fluoride, chloride, bromide, nitrate, chlorate or iodate, with chlorine, bromine, and iodine; or sodic chloride with those elements.

The electro-motive force of zinc in chlorine water, with platinum in solution of potash, the liquids being one-fourth saturated and separated by a parchment diaphragm, as determined by balancing it by the thermo-pile, was 1·2644 volt. With platinum in each liquid it was ·944 volt, and with zinc in each liquid ·7067 volt, the liquids in each case being at 18°C.

The following substances were omitted from the above list :—

	TEMPER. AT DEGREES C.	AVERAGE VOLTAIC ENERGY.
HF+Cl	at 22	7,140,000
HF+Br	20	1,817,901
Dextrotartaric acid+2 Cl	21	1,708,454
HNO^3+Br	20	1,312,000
Br+HBr	10	908,950
HF+I	19	493,333
HBO^2+Br	16	272,685
HI (pure and colourless)	11	81,800
Racemic acid+H^2CrO^4	19	10,148
AmBr+AmCl	9	73
K^2CrO^4+K^2SO^4	15	−1,573

V.—*The Glaciation of Greenland.*

By Wm. Sherwood.

[Read before the Geological Section, May 8th, 1890.]

The primary causes of the glaciation of Greenland are not far to seek. It is bathed on both sides by cold oceanic currents, returning from the Polar Basin laden with ice. The high latitude and extremely mountainous character of the country have caused the icy mantle put on in glacial times to be retained. Nothing was known of the interior until Dr. Hayes, Professor Nordenskjold, and Mr. Peary penetrated into it at different latitudes from the west coast. They always found the ice sheet gently rising towards the east; although, in one case, over 200 miles from the coast was reached at an altitude over 7,000 feet. Dr. Nansen crossed the continent in 64°30′ N. latitude, from east to west, where, from shore to shore, it is about 260 miles. At 40 miles from the east coast the altitude was about 7,000 feet, and he then attained what he terms a flat and extensive plateau, at about 8,500 feet, evidently the divide between the east and west side. He states that the ice sheet still continued to rise towards the north, so that he was simply crossing an ice stream flowing from a higher level in that direction. Although this plateau appeared flat, there must be a point where the stream divided, one fanning out to

the west, and the other to the east side; and, as this dividing snow shed occurs much nearer the east, it will account for the greater number of icebergs found on the west side. Taking into account the considerable altitude attained by the other explorers further north, where the continent is some 200 or 300 miles wider, with a rising snow sheet to the east, and the upward slope towards the north, seen by Dr. Nansen, it points to the fact that the highest altitude of Greenland will be found to exist on the north-east side. Should there be any dominating peaks rising much above the general level of the surrounding submerged summits, they will no doubt pierce the ice sheet and appear above as nunitaks; but if the altitudes of the mountains are more uniform or dome shaped, then, if we pursued the upward slope, we should simply arrive at the main centre of dispersion; from which point the ice streams would flow down at a gentle angle in all directions, until they were diverted and carried along with more powerful currents below. Under such centres of dispersion it appears improbable that there can be any vast depth of ice; but, on the contrary, the mountain summits will most likely be comparatively near the surface. As the surrounding ice cannot run uphill to the centres, and as the wind rather takes away than adds to the highest and most exposed levels, it follows that the only accumulations possible there would be from direct precipitation. As this would in course of time reach that point which the laws of ice movements allow, all beyond that limit would flow downwards; more rapidly in times of greater, and less rapidly in times of lesser precipitation. This would keep it at its maximum depth, until the climate below became less rigorous, when the ice in the valleys would sink, and give a more acute angle to the ice slopes above. These would in their turn sink also, and expose the mountain summits underneath, causing nunitaks in the early, and valley glaciers in the later, stage. In the early stage of the ice submergence of Greenland, it would no doubt increase more rapidly until the valleys and depressions were filled up; but when once the mountain ridges

were overtopped, the upper currents would have free play to flow down direct, so the thickening of the ice sheet would proceed less rapidly in consequence.

The lower currents still continue to do their utmost to relieve the interior of its burden, and ice movements are found to be far more active at the entrance of the submerged valleys than in positions away from them. The Jakobshavan Glacier, which appears to flow out of a main valley tapping the interior ice, has been reported to move in summer, 50, 65, 70, and even 99 feet a day. At the same time, the ice sheet a few miles away was found to flow only at the rate of 16 feet a day. The latter speed is very great, in comparison to the moderate rate at which the glaciers from the smaller ice sheets in Switzerland and Norway travel. It points in this direction, that the greater the pressure from behind, the greater the speed in front, that the lower ice is squeezed or forced out through the deep valleys or channels of drainage ; while the upper currents overtopping the mountains simply run down direct from centres of dispersion, or, at any rate, are not subjected to such great pressure from behind, as that in the deep valleys.

Without the aid of mountain barriers, acting as a dam or stay, it appears reasonable to suppose we should not get the vast depths we now find in the interior, nor is it probable that it could ever have been much, or any, deeper in glacial times. It would extend further all round if the sea were not deep enough to float it. I do not see that this would materially increase the depth at high centres of dispersion. The Polar Basin is at present a storehouse for cold, where the warm water from equatorial latitudes is carefully excluded, except in one direction from the Atlantic by way of Spitzbergen. The passages into the Polar Basin, by way of Smith's Strait and Lancaster Sound, are narrow, and the currents flow outwards instead of inwards ; while Behring Strait, the sole communication it has with the Pacific, is both narrow and shallow ; so currents from this direction are practically cut off altogether, and are turned back without entering it.

Both winds and water from the large area of ice and cold bring a low temperature, and help to render Greenland still a highly glaciated country,

Although the warm sea currents do not come sufficiently near to materially melt her snows, yet they assist in keeping the sea open on each side of her ; thus, moisture arising from them is blown across the high land, which is precipitated as snow, adding to, instead of diminishing, the accumulations.

It is clear that it is not one, but a combination of circumstances, operating in the same direction, that has reduced Greenland to her present condition, and that she can only be relieved of her burden by a like combination of causes, operating in the opposite way, which may, in course of time, bring about such alterations in her climatic conditions as to enable her to cast aside her winter garments and put on her summer dress once more, such as she wore before her glacial age set in.

The first important step in this direction, is a subsidence of polar lands, not only reducing the area in Greenland above snow line, which forms the feeding ground for snow and ice, but deepening and widening the present channels which connect the circumpolar sea with the Pacific and Atlantic Oceans ; thus admitting a greater volume of warm water in on both sides and allowing a free passage for ice to escape which is now confined within it. With this influx of warm water a general rise in the temperature of the Polar Sea would follow, dissolving what ice there was left ; and, instead of it being a storehouse for cold, it would become a storehouse for warm water and atmosphere. The latter would be blown across Greenland as a warm, instead of a cold, wind, as at present ; and, instead of the sea currents returning as they do now chilled by the cold and ice gathered on the journey, they would lap the shores of Greenland on both sides at a more genial temperature, driving the snow line high up the mountains, and exposing the land below to the influence of solar rays, to be partly absorbed and retained instead of being used up for melting purposes only, or reflected back again into space, as was the case when ice covered the ground. The

wind which now prevails from the cold region, would give place to southerly warm winds, which are now the least prevalent. Thus, these changes would go on acting and re-acting upon each other for long periods, until at last they would work out great results, and extend their influences over larger and still larger areas, all of which would affect the climate of Greenland to a greater or less degree; and geologists of the far future, should they continue to exist, may express surprise when meeting with evidences of the extreme glaciation Greenland once underwent.

The favourable effect with its winter occurring in perihelion, with the present slight eccentricity of our earth's orbit, must be so small, in comparison to geographical and physical causes acting the other way, that its influence, I think, must be inappreciable.

VI.—*On the Study of Earthquakes in Great Britain.*

By Charles Davison, M.A.

[Read before the Society, February 5th, 1890.]

There can be little doubt that the more important contributions to our knowledge of earthquakes must be made in countries like Switzerland, Italy, and Japan; countries where earthquakes are frequently occurring, where occasionally they are so disastrous as to arrest universal attention, and where at the same time there are many skilled observers, aided by a sympathetic and intelligent public. In England, as every fairly strong shock shows, there is no lack of observers. But our earthquakes that are strong enough to attract general notice within the disturbed area are few and far between. If we exclude special districts, like Comrie and the Durham coast, we shall probably be well within the mark in stating the average number recorded as less than one a month.

The number of earthquakes that occur in Great Britain must, however, be far greater than this. From various causes, many shocks that are felt are never placed on record. Others, again, that might be felt must certainly pass unnoticed, for, wherever seismic studies are newly organised, it is found that

people become educated in detecting earthquake shocks. But however skilful observers may become, there must always be a large number of shocks that never could be felt, either from the small amplitude or the long period of their vibrations. Even in Tokio, where they talk about earthquakes as we in England talk about the weather, "the majority of shocks pass unfelt by people, while seismographs register them sufficiently to allow measurements." *

There is every reason to conclude, then, that, with the aid of simple time-recording instruments, the earthquakes of Great Britain would be found sufficiently numerous to repay a more careful and systematic study. That we shall have to be content as a rule with observing shocks that would elsewhere be considered slight is evident, of course; but in their very feebleness we possess advantages which are not afforded by the severer shocks of other lands. Not only are the phenomena much less complex; but, not being unnerved by danger, the observer is able to concentrate his attention on them more calmly and completely. Still more important is the fact, and in this lies their greatest value, that, the smaller the area disturbed, the more nearly can the position of the epicentrum be determined. If, as is frequently the case, the shock be felt only within a small circular area, we cannot be far wrong in regarding the centre of that area as the approximate site of the epicentrum. And thus we easily obtain the solution of what, in a great earthquake, is one of the most difficult and important of the problems to be attacked.

Methods of Study in Great Britain.—Owing to the feebleness of our shocks, and their comparative rarity in a given district, the methods of study employed by us must clearly be different from, and inferior to, those adopted with such signal success in Italy and Japan. We can hardly expect, for instance, that costly recording instruments will be widely used in this

* S. Sekiya. Japan Seism. Soc. Trans., Vol. X., p. 59.

country; for, even in Italy, as Professor de Rossi points out,* it has been found better to have a large number of observatories containing cheap and simple instruments, than a few equipped with seismographs more perfect and refined.

If, on the one hand, then, our methods of earthquake-study are limited by the nature of the shocks we experience, on the other we possess advantages, apart from those already mentioned, that are more or less wanting in regions where the phenomenon attains a more destructive and interesting development. For instance, most parts of England, at any rate, are so densely populated that we are able, almost wherever a shock occurs, to procure a large number of observations of very considerable value. And, again, in the ease and accuracy with which we can regulate our clocks in the neighbourhood of every large town, we have an aid in our work which is as valuable as it is rare in foreign countries. These two facts in particular I mention here, because they form the foundation of our two most promising methods of investigation.

Looking at earthquakes chiefly from a geological point of view, that is, regarding them as mere incidents, but at the same time delicate indices, of the progress of terrestrial evolution, the prime object of our enquiries is in every case to determine the position of the epicentrum, and, if possible, that of the seismic focus. For this purpose, we have three methods at our disposal, depending severally upon observations of the direction, intensity, and time of occurrence, of the shock in different parts of the disturbed area.

The first method is interesting historically from its having been used by Mallet in the earliest scientific study of an earthquake. But modern seismologists have, with good reason, generally discarded it; and, in any case, it could hardly be employed with success in this country.

The method of intensities is far more reliable, and is attended with good results whenever the observations are

* Bull. del Vulc. ital. anno IV. (1877), p. 5.

sufficiently numerous and made at places that are fairly evenly distributed over the disturbed area. With the aid of such a scale as that drawn up by MM. de Rossi and Forel, the intensity of the shock at any point may be roughly estimated. Then, drawing lines including all places where the intensity is at or above a certain degree of the scale, we obtain a series of lines of equal intensity (isoseismal lines), which, closing in towards the epicentrum, enable its position to be approximately determined. For the slighter shocks that we experience it would be difficult to over-estimate the value of this method, the only one that, in certain cases, can be applied.

The last of the three methods, I think I may say, is still upon its trial; and if, so far, it has not yielded the results that are to be expected from it, I believe the reason is that it has not yet been attempted in a country where the conditions are so favourable for its application as they are in many parts of England. What we require for the purpose is not a network of time-recording instruments extending over the whole country, so much as a moderate number suitably placed and regularly observed in specially selected districts. If, by means of these instruments or otherwise, the times of a shock can be ascertained with accuracy at five or more places; these, under certain conditions, are theoretically sufficient to determine the position of the epicentrum, the depth of the seismic focus, the velocity of the earth-wave, and, consequently, the time of occurrence at the focus; and it should be noticed that time-recorders in Great Britain are practically free from the objection which attends them in Japan and other regions where earthquakes frequently last for one or several minutes. For, in such cases, the character of the shock varies so greatly throughout the disturbed area, that it need not, and probably will not, be one and the same vibration which is registered in different places, and considerable errors may thus be introduced.* If,

* E. Knipping and H. M. Paul. Japan Seism. Soc. Trans., Vol. VI., p. 37 also J. A. Ewing, Vol. III., pp. 63-4; and J. Milne, Vol. IV., pp 100-1.

then, we remember that our earthquake-shocks seldom last for more than a few seconds at most, and that, in many parts of England and some parts of Scotland, it should be possible to ascertain the time of occurrence correctly to within a small fraction of a minute, I think there can be little doubt that, for all but the slightest shocks, a most fruitful method of earthquake study in this country would be a system for securing accurate time-records whenever a shock is felt.

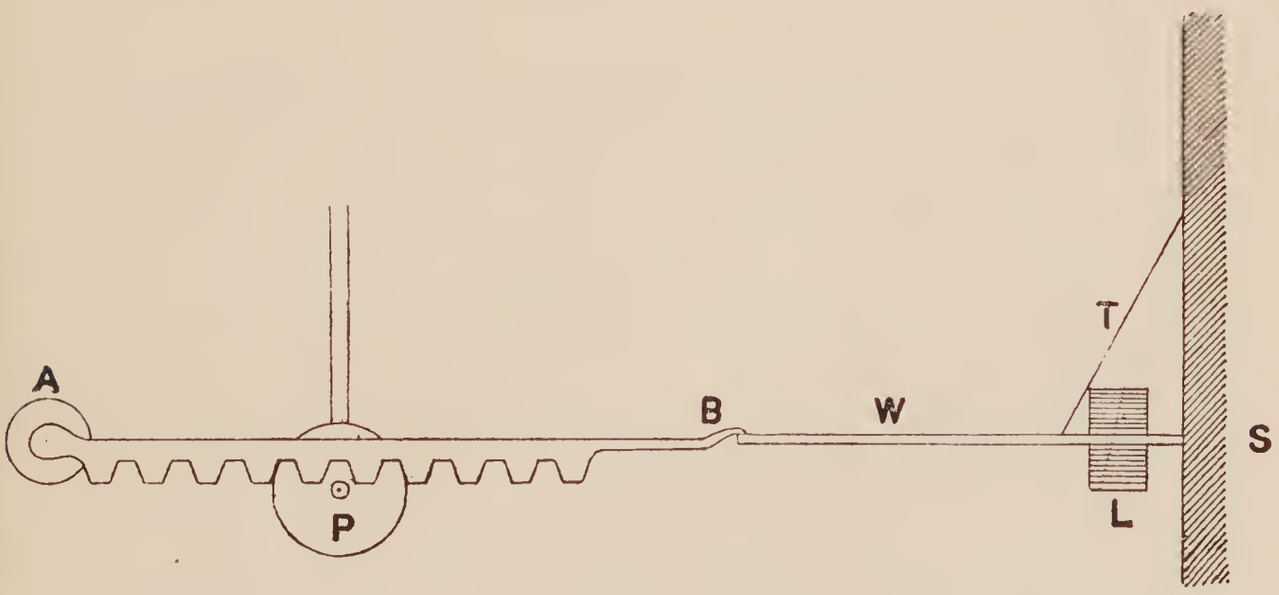
Seismoscopes.—A large number of simple and inexpensive seismoscopes have been devised and used for recording the time of occurrence of an earthquake shock; but it is difficult to find one that in all respects is thoroughly satisfactory.* To be so, they should fulfil the following conditions:—They should be inexpensive, simple in their construction, easy to arrange, and require little attention when once erected. They should record the occurrence of shocks and tremors with equal facility in whatever direction they may arrive; and they should be equally sensible in recording a feeble shock. It is very desirable also that they should be of similar construction, at any rate in a given district, if not throughout the whole country, so that observations from different places may be rightly comparable.

Again, in countries where earthquakes are frequent, and where the shocks may succeed one another at short intervals, it is important that the record should be made without stopping the clock. In Great Britain, however, our catalogues show that, except at Comrie, it is not usual for sensible earthquakes to follow one another rapidly, and it is therefore worth while considering whether, on account of their much greater cheapness and simplicity, it might not be well to avail ourselves in this country of clock-stopping apparatus. Such instruments are, of course, defective in that, until re-set, they are incapable of recording a second earthquake But they possess a compensating advantage in the accuracy with which it is possible to time the occurrence of a shock.

* A Committee of the British Association is at present engaged in considering the form of seismoscope most suitable for use in this country.

Clock-Stopping Apparatus.—As it is possible to make with ease, and at little cost, an extremely delicate apparatus for stopping a clock at the moment of a shock, I quote the following description of one devised by Professor J. Milne.*

"P is the pendulum of a clock with a small piece of wire standing out at right angles to its face. . . . This wire as the pendulum swings, passes beneath a series of teeth cut in a strip of wood lightly hinged at A and terminating at the other end B with a piece of stiff wire. . . . If such a contrivance is allowed to fall, the teeth catch in the projecting pin of the pendulum and it may arrest it at any portion of its swing." The arrangement which, at the time of an earthquake, allows the toothed lever to fall, "consists of a piece of stiff wire W on which, near to one end is a small cylinder of lead L. The short end of this wire is pointed, and rests in a pivot-hole made in the head of a drawing pin pressed into the side of the clock case S. To prevent this wire from falling it is held up by a small silk thread T fastened to a second drawing pin. As suspended it is very unstable and instead of remaining at right angles to the clock case, it swings round against it. When however the wire B rests on the end of W, it retains its position as shown in the figure."



* Japan Seism. Soc. Trans., Vol. III., pp. 61-2.

This instrument is so sensitive that it is difficult even to shut the clock case without stopping the clock. The reason of this appears to be "that if the clock case receives a small displacement at right angles to W, the weight remains steady by its inertia, whilst the long arm of W in contact with B multiplies the initial motion" approximately in the proportion of the length of the long to that of the short arm of W.

It would appear that a displacement parallel to the wire W would not give this multiplication; but, practically, Mr. Milne observes, "it seems impossible to give a motion in that direction to which the apparatus does not seem to be just as sensible as to a motion in any other direction. The only other motion which does not result in stopping the clocks appears to be a *very slow* easy swing," and thus the instrument will probably be incapable of recording the occurrence of the dying-out vibrations of a very distant shock.

The instrument may be placed in a cellar or out-house, or out of doors under cover of a close-fitting box. A strong stake should be driven into the ground to the depth of two or three feet, the floor, if any, being removed for a few inches round the stake, to prevent the instrument being disturbed by the vibrations of the house. The clock-case should then be secured firmly to the stake.

If several of these instruments are erected in a district, they should be placed at distances of not less than 5 to 10 miles apart.* The sites selected, should, if possible, be free from the vibrations of passing carts and trains. If two or three of them record the occurrence of a shock at very nearly the same instant, it may be inferred that the disturbance is not accidental

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* If the clock be carefully rated, it should be possible to obtain the time of a shock correct to a tenth of a minute. The velocity of earth waves is subject to wide variations, even in traversing the same rocks, but, taking it 1,000 feet per second, it follows that the earth-wave will pass over more than a mile in one-tenth of a minute. A good deal more than a mile, then, should separate every pair of stations where seismoscopes are placed.

in its origin, and the inference will be strengthened if several instruments closely agree in their indications. But a record from one alone must obviously be regarded as doubtful, if all the others were at the time in good working order.

Suggestions for the Observation of Earthquakes (without the use of special instruments).—Lists of questions for aid in the study of earthquakes have been drawn up by Professor Heim and Professor Milne.* The following questions are founded partly on these lists, but chiefly on the accounts of earthquakes in different places, and especially in this country. It is hardly necessary to insist that all notes should be written down on the spot, or as soon after the shock as possible; but it may be useful to remark that it is often just as important to note when a given phenomenon is *not* observed as to describe it fully when it is observed. This applies particularly to the mere fact of the perception of the shock, as a knowledge of the places where it is just not felt is of service in enabling us to determine the boundary of the disturbed area.

The questions are arranged in the following sections: A, for places where the shock is felt; B, for those where it is not felt; and C, inquiries to be made after the shock. In each case, the questions to which it is most important that answers should be given are marked with an asterisk.

A.—For Places where the Earthquake is Felt.

1. *Place of observation.*

* *a.* Its name and position.

b. Nature and form of the surrounding ground, especially with reference to its geological structure and the neighbourhood of mountains, rivers, cliffs, etc.

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* A. Heim, Die Erdbeben und deren Beobachtung, pp. 27-9; Arch. des Sc. phys. et nat. 3me pèr. t. III., pp. 286-7; Fouqué, Les Tremblements de Terre, pp. 133-4; Japan Seism. Soc., Trans. Vol. I., pt. II., pp. 3-4.

2. *Situation of observer.*

* *a*. Whether indoors or in the open air; if indoors, on which floor of the house.

b. If indoors, the direction of the street, or of the longer axis of the house (if detached).

* *c*. How occupied at the moment of the shock—lying down, working, etc.

3. *Time of occurrence.*

* *a*. Time at which the shock was felt; if possible, to a tenth of a minute.

* *b*. Is the time given the correct time obtained after comparison with an accurately rated chronometer; and, if so, how long after the shock was the comparison made; and how is the chronometer regulated?

4. *Nature of shock.*

* Describe the nature of the shock as closely as possible, stating especially (*a*) the number of the more prominent vibrations; (*b*) their relative intensity; (*c*) whether there was any tremulous motion before or after the vibrations; (*d*) whether any vertical motion was perceptible.

5. *Duration of shock.*

* Total duration, exclusive of that of the sound-phenomena (stating whether estimated or determined by a watch).

6. *Intensity of shock.*

* Was the shock strong enough (*a*) to make windows, doors, fire-irons, crockery, etc., rattle; (*b*) to cause the chair or bed on which you were resting to be perceptibly raised or moved; (*c*) to make chandeliers, pictures, etc., swing, or to stop clocks; (*d*) to overthrow ornaments, vases, etc.,

or cause plaster to fall from the ceiling; (*e*) to throw down chimneys or make cracks in the walls of buildings ?

7. *Direction of shock.*
   a. Direction of the principal shock or shocks.
   b. Means by which the direction was ascertained.
   c. Was any change of direction perceptible during the earthquake ?

8. *Sound-phenomena.*
   * *a.* If any rumbling sound was heard at the time of the shock, what did it resemble ?
   * *b.* Did it precede, accompany, or follow the shock ? (Times useful, especially the intervals between the beginning of the shock and of the sound, and between the ending of the same; stating whether estimated or determined by a watch.)
   * *c.* Duration (given by 5*a* and 8*b*, if not determined separately).

9. *Effect on the water of ponds, etc.*
   Were any movements observed in the water of ponds, rivers, lakes, or the sea at, or shortly after, the time of the shock; if so, of what kind ?

10. *Accessory shocks.*
   * Were there any slight shocks preceding or following the principal shock or shocks ? If so, a list of these, with the place of observation, time of occurrence, and answers to any of the above questions, would be of great value.

B.—For Places where the Earthquake is Not Felt.

1. *Place of observation.*
   * *a.* Its name and position.
   * *b.* Nature and form of the surrounding ground, especially with reference to its geological structure and the neighbourhood of mountains, rivers, cliffs, etc

2. *Situation of observer.*

* *a.* Whether indoors, or in the open air; if indoors, on which floor of the house?

* *b.* How occupied at the moment of the shock—lying down, working, etc.

C.—Inquiries to be Made After the Shock.

1. *Damage to buildings.*

a. Nature of the building damaged.

b. Situation of the building, direction of its longer axis; neighbourhood to the edge of a cliff or bank, and on which side of this it lies; nature of the rock on which it rests.

c. If any cracks formed, state in which walls; the direction and width of the cracks, and the points from which they start (sketches useful).

d. If it is noticed that some walls are much damaged, while others at right angles to these are but little affected, what are the directions of these walls?

2. *Rotation of objects.*

* *a.* If objects such as chimneys, gravestones, gate-pillars, etc., have been rotated on their bases during the shock, describe the initial and final positions of the objects (sketches useful); or state the direction and amount of the rotation (looking down on the object from above, is the direction the same as that in which the hands of a watch rotate, or opposite to that direction?).

b. Is there any evidence of rotation in bodies with a circular base?

3. *Effect on the ground, springs, etc.*

a. Were any fissures or cracks formed in the ground? If so, state their length, width, depth, and direction, the nature of the ground in which they occur, and their relation to neighbouring cliffs, banks, etc.

b. Was the height, quantity, or temperature of the water in springs affected by the shock?

4. *Observations in mines.*

If the earthquake was felt in a mining district, inquiries should be made as to the nature of the shock and of the sound-phenomena when observed by men in the mines; the depth of the workings in such cases, etc.

5. *Records of self-registering instruments.*

* An examination should be made of the records of self-registering instruments within or near the disturbed area; particularly of recording barometers, magnetic and tidal apparatus; with a view to determine the effects of the shock on these instruments, and also to ascertain by their means the exact time of occurrence.

While answers to any of the above questions would be useful in the study of an earthquake, especial pains should, if possible, be taken to determine accurately the time at which the principal shock occurs. Immediately it is felt, the time should be noted to the nearest second and written down at once, a few seconds (to be ascertained by trial) being allowed for taking out the watch and reading off the time. As soon afterwards as possible, the watch used should be compared with an accurately regulated clock. But if this cannot be done, if the record cannot be relied on as correct to within a small fraction of a minute, a less close approximation cannot, as a rule, possess much value. The chief use of such a record is then to determine the epoch of a shock; and, in a matter of this kind, when two consecutive shocks in a given district may be separated by an interval of several years, a question of a few minutes, more or less, is of very little moment.

Next in importance to time observations are those on the intensity of a shock. Without the aid of delicate instruments it is of course impossible to estimate the intensity with accuracy

But good results have been obtained by the use of a rough scale, according to which the intensity is determined by its effect on men and their dwellings. The following is the Rossi-Forel scale,* which is widely adopted by Italian and Swiss seismologists.

I.—Microseismometric shock: noted by a single seismograph, or by some seismographs of the same model, but not moving several seismographs of different kinds; the shock felt by an experienced observer.

II.—Extremely feeble shock: recorded by seismographs of different kinds; felt by a small number of persons at rest.

III.—Very feeble shock: felt by several persons at rest; strong enough for the duration or the direction to be appreciable.

IV.—Feeble shock: felt by persons in motion; disturbance of movable objects, doors, windows, cracking of ceilings.

V.—Shock of moderate intensity: felt generally by everyone; disturbance of furniture and beds, ringing of some bells.

VI.—Fairly strong shock: general awakening of those asleep; general ringing of bells, oscillation of chandeliers, stopping of clocks; visible disturbance of trees and shrubs. Some startled persons leave their dwellings.

VII.—Strong shock: overthrow of movable objects, fall of plaster, ringing of church bells, general panic, without damage to buildings.

VIII.—Very strong shock: fall of chimneys, cracks in the walls of buildings.

IX.—Extremely strong shock: partial or total destruction of some buildings.

X.—Shock of extreme intensity: great disasters, ruins, disturbance of strata, fissures in the earth's crust, rock-falls from mountains.

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* Arch. des Sc. phys. et nat. 3me pèr. t. XI., pp. 148-9. Fouqué, Les Tremblements de Terre, p. 22, (foot note); Bull. del Vulc. ital. anno IV. (1877), pp. 39-40.

Results to be expected.—It may be useful, in conclusion, to point out some of the results we may expect to obtain from a systematic study of earthquakes in this country.

The mere indication of the occurrence of a shock felt at a given place on a given day is of service in the compilation of earthquake statistics, and will tend to give completeness to our seismic record. With the help of such a record, we can study the laws of the periodicity and geographical distribution of earthquakes. The time is past for drawing up chronological tables of shocks felt over the whole earth; but the importance of making our records complete for a definite area of study is becoming more and more evident.

The accurate determination of the time of occurrence in different places is of the very highest importance. Such observations, if sufficiently numerous, will help us in investigating the position of the area which constitutes the epicentrum; the way in which the vibrations are propagated outwards from the epicentrum; the velocity of the earth-wave, and the laws according to which the velocity varies with its distance from the origin. A knowledge of the time will also determine the question of the coincidence of shocks in distant areas, separated by a region in which the shock is not felt at all, and of other phenomena which may seem to be more or less intimately connected with the earthquake.

By a study of the intensity in the different parts of the disturbed area, we are enabled to draw one or more isoseismal lines with a fair approach to accuracy. From the form of these lines, we can ascertain the approximate position of the epicentrum; and, from the relative distances between consecutive pairs of such lines, we can determine the way in which the intensity decreases as the earth-wave radiates from the origin, and the relations of this decrease with the form and geological structure of the ground.

The chief point to which our researches at present tend is thus the discovery of the position of seismic foci. But our

ultimate object is something higher than and beyond all this. With certain exceptions, the slightest earthquake that occurs must indicate the site and mark the epoch of a step in the process of terrestrial evolution. To determine the laws of seismic distribution in space and time would therefore be to discover in part the laws that regulate the development of the earth's great surface-features. The study of earthquakes is fascinating enough in itself, but it acquires a loftier significance when viewed in its wider relations; for through it we may press forward to the solution of the great problem of geology—the origin and growth of mountain-chains.

References.

The first of the following lists contains the names of some of the more important memoirs that have been published on British earthquakes. In the second list are given the principal authorities that have been consulted in the preparation of the present paper.

A.—*Memoirs relating to British Earthquakes.*

Lebour, Professor G. A.—On the breccia-gashes of the Durham coast and some recent earth-shakes at Sunderland. Trans. N. of Eng. Inst. of Min. and Mech., Eng. (read June 14th, 1884), Vol. XXXIII., pp. 165-174.

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MAP OF THE MIDLAND DISTRICT

Shewing the distribution of Boulders

BY FRED. W. MARTIN, F.G.S.

SCALE. 2 MILES TO 1 INCH.

1890.

Newport.

Lilleshall

Shrewsbury.

River Tern.

Wellington.

1335 The Wrekin

402

Eaton Constantine

750

Little Wenlock

150

300

150

Much Wenlock.

Burton

Longville

Brown Clee Hill.

River Corve

Eaton Brook

Ledwicke Brook

Madeley

Shifnal

Tong.

Albrighton.

Beckbury

Bridgenorth.

River Worf

River Severn

Bobbington Brook

Enville

Kinver Edge

River Stour

Bewdley

Stourport.

River Severn

River Teme

Stafford.

Colwich + 235

Colton

Brocton 500

Bednall

Cannock

Rugeley

River Trent

Penkridge

Wheaton Aston

Chase

600 Hednesford

500

Cannock

477

Hednesford Hills

Alrewas

Lichfield.

River Tame

Brewood

458

Shareshill

600

360

Codsall

Bushbury 589

Bloxwich.

Pelsall

Tettenhall.

519

320

Wolverhampton.

Walsall.

Sutton Coldfield.

Wednesbury.

Erdington

Gravelly Hill

Sedgley

Tipton.

Penn 600

Seisdon

Trysull

Dudley.

Soho.

Rowley Regis.

Birmingham.

444

Stourbridge.

Halesowen.

Harborne.

325.

Kinver Edge

Churchill

Hagley

Clent Hills

Stirchley 480.

Solihull.

900

Romsley

Frankley Hill

790

450

Northfield. 500

+ (Griffel Granite)

Kidderminster.

Belbroughton 400

574

600

Madely

The Lickey Hills

862

Knowle.

Woodcote

300

Hartlebury

Stourport.

300

Bromsgrove.

309

River Salwarp

Droitwich.

Redditch.

River Arrow

+ (Griffel Granite)

Worcester.

River Avon

NOTE. *Boulders from the English Lake District are indicated thus* — o

" " *Mt Criffel and South Scotland* " — +

" " *Mt Arenig and North Wales* " — ▪

Local Rocks such as Rowley Basalts and Sandstones " — ·

Ordnance Survey levels are indicated by figures " — 519

The localities marked by a ? have not been Surveyed but it is most probable that boulders exist there.

VII.—*The Boulders of the Midland District.*

(Being a Second Report.)*

By Fred. W. Martin, F.G.S.

[Read before the Society, February 5th, 1890.]

The accompanying map is a rough map of the country north, west, and south-west of Birmingham, to a scale of two miles to one inch, taking in Lichfield, Rugeley, Penkridge, the Wrekin, Much Wenlock, Kidderminster, and Bromsgrove. It indicates the general distribution of boulders over that area so far as I have been able to ascertain it from survey of the ground. The *crosses* indicate boulders supposed to be derived from South Scotland, principally grey granites from Mount Criffel. The *circles* indicate Lake District rocks, including coarse and fine granites from Eskdale, syenites from Buttermere, and various felsites and andesites, ashes and lavas. The *cubes* represent Welsh boulders, consisting mostly of felsitic ashes from Arenig, and slates and shales. Finally, shown by *large dots*, is a local distribution of basalts broken from the hills of Rowley Regis, and scattered both round their bases and about Birmingham; these consist of angular and subangular blocks.

The whole of the ground north of Kingswinford is fairly covered with boulders of English Lake and Scotch origin, but very few of such occur south of this. There is no distinct line of termination of the deposit, such as a range of hills would form. I suggested in my first report that the Cannock hills seemed to have formed an effective barrier, but I have since found that such is not the case; boulders lie on the top of these

* The First Report was published in the "Proceedings of the Birmingham Philosophical Society," Vol. VI., p. 93, having been read before the Society on May 10th, 1888.

hills at a level of 648ft., and they are thick upon the ground on the other side of the hills near Hednesford. The Hilton hills also are close to the margin of the boulder deposit, but numbers lie beyond them round about Essington. Boulders lie upon the high ground at Walsall and Penn, and many have passed over the Abbot's Castle Hill into the Enville country, and may be found round farm buildings and pools, as, for instance, at Morfe Heath, where there is a very large deposit. In fact, I have every reason to believe that if the ground in this neighbourhood were more cultivated numbers more would be brought to light, but the country is very hilly and railway accommodation bad, so that there is practically no means of getting rid of the farm produce, besides which a large area is occupied by the pebble beds, upon which nothing but fern and heather will grow. Therefore, except here and there, the ground is never disturbed, and the boulders, if they are there, lie quietly beneath the surface.

We shall find ample confirmation of this statement if we compare the state of things at Enville with that at Much Wenlock and Burton. Here we have hilly country again, but there is a railway running along the foot of the Wenlock Edge, and the land on its flanks is cultivated, consequently we find boulders in immense numbers. Curiously enough, some of these boulders lie close on the edge of the precipitous side of the hill, where perhaps they have been rolled; though as they are of large size they cannot have been moved many yards from where they were originally found.

Again, several boulders may be found about half way up the steep hill between Buildwas and Little Wenlock, and also in the village of Little Wenlock.

Passing on to Cannock Chase, one may walk over acres of moorland without seeing a sign of an erratic; but where a cottager has dug for himself a garden, however small, boulders will be found in the hedge bank that surrounds it.

I chanced to come across some new cottages between Hednesford and Cannock Norton, where the ground had been

once dug over to form gardens, and the surface was literally white with bleached granites and trap rocks, from the size of pebbles up to 9in. diameter, which, for easy reference, I call "Boulder Gravel."

On a sunny day this bleached gravel may be seen scattered over the ploughed fields round Penkridge, Lapley, and Wheaton Aston, standing out in striking contrast to the dark soil upon which it rests, and may readily be distinguished from ordinary Bunter pebbles. So far as I have been able to ascertain, the boulders do not appear to be more numerous on one level than on another; they seem to cover hill and dale alike.

In some villages, as, for instance Shareshill, Seisdon, Trysull, Codsall, &c., the boulders are so numerous that the hedge banks and foundations of walls are built of them, or the streets paved with them; and others, too cumbersome to move, lie at the lane corners, or guard the gateways to farm yards. In one or two cases they are used as mounting stones. I believe that if villages were to be established anywhere within a radius say of ten miles round Codsall, enough boulders would be got out in excavating for foundations to pave the whole of their streets.

The shapes of the boulders vary; the majority of them, perhaps, are subangular, the angles being more or less rounded, whilst many, more especially, I think, the Scotch granites, are very much rounded. Several of the andesites and syenites have a very smooth surface, and are sometimes flattened and smoothed on one side. Some are distinguished by their angularity, although the rough edges are often rounded off. Others of the granites, especially the very large ones, are more of a rectangular shape, with very little rounding off at the angles.

The deposit of Welsh boulders round the Lickey is entirely distinct from the collection of English and Scotch rocks at Wolverhampton. Whether or not they came down at a different period is a question for determination. There are very few rounded blocks amongst them, most of them being rough and

broken, apparently little altered since they left the parent rock. They were deposited anywhere, irrespective of hills, and are scattered over most of the ground between Bromsgrove and Harborne. It would be interesting work to trace back these Welsh boulders to the Arenig Hills, from which we suppose them to have been derived, and also to hunt for further traces of the boulder clay or "till," such as we find on the Frankley Hill at a high level.

Nearly all the erratics mentioned in my catalogue have been found upon the surface of the ground, and not in drift clays or gravels.

As to chalky boulder clay, I have found around Albrighton and Penkridge, and elsewhere, fragments of chert and limestone containing fossils, which may have belonged to the chalky boulder clay, but I have preferred to suppose that any fragments I found have been thrown upon the land by farmers for agricultural purposes.

Then, as to the levels at which boulders are found, I said before, they seem to have been deposited wholesale, and without regard to elevation of the ground, nor are they confined to one side of a hill, but are to be found as often on the east side as on the west.

Very few local rocks seem to be intermixed with the northern erratics. Now and then a block of sandstone or basalt may be seen, but these are at rare intervals.

I might be asked if there are any localities which are boulderless? This would be a difficult question to answer. The deposit of English Lake and Scotch rocks certainly seems to thin off to the east of a line drawn between Sutton and Lichfield, and south of a line between Enville, Dudley, and Walsall, but the surface of this latter ground has been so much altered since the glacial period, that one would not be safe in saying that no boulders exist beneath the modern hills of the Black Country; but, as I have previously said, most of the ground north and north-west of these lines is more or less strewn with erratics.

I have found no Welsh boulders north of Stourbridge, or south of Stoke, though they extend eastwards as far as Sutton, and they are most numerous near Frankley and Northfield. The large blocks in this district have arrived in just the same state as when they were broken from the parent rock, their torn edges unrounded, their rough surfaces unsmoothed, also with no glacial markings; the small blocks, say of a pound weight, such as are found in the boulder clays, are striated and smoothed; but I have not included such as these in my catalogue, as special accounts have been given of them by Dr. Crosskey.

The map is by no means complete, large tracts of country still being uninvestigated; but I think sufficient has been done to bring out a curious result. Upon reference, it will be noticed that the central portion of the map is occupied by a body of northern erratics, which seem to fringe off on a line curving from Much Wenlock round Burton, Bridgnorth, Enville, Harborne, Erdington, Sutton, Burntwood, to Colton, and that outside this body of northern rocks is a concentric deposit of Welsh ones, having on the outer edge Chaddesley Corbett, Bromsgrove, Tardebigge, King's Norton, Erdington, and Sutton Coldfield. The only inference that can be drawn from this state of things would seem to be that the Welsh boulders were deposited first, and that what we find now is only a marginal remnant of a much larger deposit, which has since been destroyed or transported elsewhere, and that at some subsequent period the northern erratics followed up but did not reach so far as their predecessors, except at Sutton Coldfield, where they have overlapped.

There can, I think, be no question here about intercrossing. The boulders we find are the remains of two distinct periods of action,

Mr. Mackintosh, in an exhaustive paper read before the Geological Society, in 1879, lays great stress upon what he considers to be an "Intercrossing of the Courses of Boulders." For instance, he says: "The Eskdale granite dispersion, which went to Burton (Shropshire) must have crossed the course of

the stream of the Criffel granite which went to the west and north of Wolverhampton." Now, I have not been able to find sufficient evidence of any such crossing. At Burton and Much Wenlock, I made notes of a large number of boulders, the result showing that thirty per cent. of those found were Criffel granites, fifty-seven per cent. Lake District andesites and felsites, and only thirteen per cent. Eskdale granites. This, I think, could hardly be called an Eskdale granite dispersion, when the Criffel granite numbered thirty per cent. of it. However, I will admit that nowhere else have I found such a large percentage of Eskdale granite. The average percentage over the country round Wolverhampton seems to be about forty-four per cent. each of Criffel and Lake District andesites and felsites. The remaining twelve per cent. being made up of Buttermere syenites, Eskdale granites, Hornblendic granites (which I am given to believe are from South Scotland) and local rocks.

On his map, Mr. Mackintosh has marked, in various places, notes of interrogation, which I take to mean that boulders *may* exist in these localities, but that he has not surveyed them. I have ventured, therefore, to supplement his work with a map showing in detail many of those parts filled in which were wanting in his map ; but, at the same time, my own map contains several vacant places, which I hope to be able to fill in eventually.

In preparing these notes I have been greatly assisted by Dr. Lapworth, who has very kindly spent much valuable time in naming specimens of the boulders found.

I have also to acknowledge my indebtedness to Mr. William Sherwood, of Sutton Coldfield, who has placed at my disposal, much valuable information with regard to the boulders of that district.

I have collected a large number of specimens which are being preserved for inspection by any who may be interested in the subject.

Since the above went to press, I have been informed by Mr. Joseph Landon, F.G.S., that he has discovered, in some

excavations for a new road at Northfield, a few Criffel Granites and several rocks from the English Lake District. Now I have not found northern boulders on the surface in that locality, and the fact that they have been found there underground not only confirms the idea that the Welsh rocks were deposited under very different conditions to those which prevailed when the northern rocks were brought down, but tends to show that the area included within the outer concentric curve above described has undergone further changes since the boulders came.

CATALOGUE OF BOULDERS.

(*Catalogue No. 2.*)

Explanation of Abbreviations.

E.L. indicates "English Lakes."
C. ,, "Mount Criffel" (Scotland).
B. ,, "Buttermere."
Sub. ,, "Subangular."

The figures within brackets refer to the level at which the particular boulders are found, and denotes approximately the height above the mean sea level at Liverpool.

The sizes of boulders given are *approximate*.

Penkridge to Stretton and Wheaton Aston.

Kinvaston (*in lane, by the Withy Pit*).

Andesite (E. L.); 18 in. diam.; rounded.
Syenite; 18 in. by 12 in.; sub.
Ibid. 24 in. by 24 in.
Granite (C.); 24in. by 24 in. by 18 in.

Otherton.

Andesite (E. L.); 36 in. by 24 in. by 18 in.; sub.
Syenite (E. L.); 36 in. by 24 in. by 24 in.; rounded.

Watling Street (*corner of lane to Engleton Hall*).

Felsite; 36 in. by 30 in. by 18 in.

HORSEBROOK.

Felsite; 24 in. by 12 in.
Granite (C.); 15 in. diam.
Ibid. 21 in. by 18 in. by 12 in.; square.
Ibid. 30 in. by 24 in.; sub.
Ibid. 36 in. by 24 in.; square.

By Stretton Mill.

Granite (C.); 30 in. by 24 in. by 18 in.

LAPLEY (*in village*).

Granite (C.); 24 in. by 24 in. by 18 in.; sub.
Ibid. 36 in. by 30 in. by 24 in.; sub.
Felsite; 24 in. by 18 in.; sub.
Two Granites (C.); each about 18 in. diam.

In lane near Swan Inn.

Andesite (E. L.); 60 in. by 42 in. by 18 in.; oblong.
Granite (C.); 24 in. by 18 in.
Granite (C.); 24 in. by 18 in.; sub.

Gravel seems to be composed largely of material from English Lakes, and contains several blocks of Andesite, Syenite, etc.

WHEATON ASTON (*just beyond the stream*).

Felsite (E. L.); 24 in. by 18 in.

Near canal.

Granite (C.); 18 in. by 18 in. by 12 in.; sub.

Entering village from Lapley.

Granite (C.); several blocks about 18 in. diam.
Ibid. (at angle of farm buildings); 57 in. by 36 in. by 30in.; ends rounded.
Ibid. 36 in. by 24 in. by 24 in.; ends rounded.
Andesite (E. L.); several blocks about 18 in. diam.; ends rounded.
Syenite (E. L.); ends rounded.
Granite (coarse Eskdale); ends rounded.

At cross-roads.

Andesite (E. L.); 30 in. by 24 in. by 24 in.; oblong.

Street near church is paved with erratic blocks, also Granite (C.); 30 in. by 24 in. by 18 in.; oblong, ends well rounded.

Along lane by Coach and Horses, in a garden.

Andesite (E. L.); 48 in. by 30 in.; sub.

By gateway.

Granite (C.); 24 in. by 24 in. by 18 in.

Syenite (Buttermere); 30 in. by 24 in. by 18 in.; sub.

By pool.

Four Granites (C.); about 24 in. by 24 in. by 18 in.

Two Syenites (Buttermere); about 24 in. by 24 in. by 18 in.

Five Andesites (E. L.); each 18 in. by 18 in. by 15 in.

Penkridge (*along road to Dunston*).

Felsite (E. L.); 30 in. by 30 in. by 24 in.

Grey Granite (C.); 24 in. by 24 in.

Andesite (E. L.); 15 in. by 12 in. by 9 in.; sub.

Ibid. 18 in. by 12 in. by 9 in.; sub.

Dunston.

Grey Granite (C.); 24 in. by 24 in. by 18 in.

Ibid. (by seat); 30 in. by 30 in. by 30 in.

Andesite; 12 in. by 12 in. by 9 in.

Granite (C.); 27 in. by 24 in. by 15 in.; sub.

Also several other Criffel and hornblendic Granites, Andesites, and Syenites built into walls, hedge banks, etc., but most of them small.

By Acton Mill.

Granite (C.); 30 in. by 27 in. by 24 in.; sub.

Acton Trussel.

Several small blocks as above.

BEDNAL (*along road opposite the church*).

Several Criffel Granites, Andesites, and Felsites, averaging about 18 inches in diameter, and about equal quantities of each.

Along road left of church.

Syenite (B.) ; 24 in. by 18 in. by 18 in. ; oval.
Granite (C.) ; 24 in. by 18 in. by 18 in. ; sub.
Andesite (E. L.) ; 21 in. by 21 in. by 18 in.

Along road beyond church.

Andesite ; 18in. diam. ; rounded.
Local Sandstone ; 24 in. by 18 in. by 18 in.
Andesite ; 15 in. by 15 in. by 9 in. ; smooth.
Four Granites (C.) ; about 24 in. by 24 in. by 18 in. ; sub.
Slate ; 24 in. by 18 in. by 9 in.
Felsite ; 18 in. by 18 in.
Syenite (B.) ; 21 in. by 21 in. by 12 in. ; sub.
Andesite (in pavement) ; 30 in. long.
Two Granites (C.) ; 15 in. by 15 in. ; sub.
Felsite ; 18 in. by 15 in. by 12 in. ; sub.
Syenite (B.) ; 21 in. by 12 in. by 12 in. ; sub.
And several others in hedge bank.

At corner of roads.

Granite (C.) ; 24 in. by 18 in. by 15in. ; sub.

Along lane, by blacksmith's shop.

Granite (C.) ; 24 in. by 24 in. by 15 in. ; sub.
Andesite (E. L.) ; 24 in. by 24 in. by 18 in. ; rough.

Along lane just out of village.

Syenite (B.) ; 21 in. by 21 in. by 15 in. ; squarish, one side smooth and flat.

At corner of lane to Bednal Head.

Andesite (E. L.) ; 36 in. by 24 in. by 24 in. ; sub.

At corner of lane and Cannock turnpike road.

Granite (C.) ; 24 in. by 18 in. by 18 in. ; sub.
Andesite ; 21 in. by 18 in. by 15 in. ; sub., smooth.

Near Brocton Gate.

Felsite ; about 9 in. diameter.

Brocton (*by cottage gardens, Brocton Lodge*).

Andesite (E. L.); 48 in. by 36 in.; triangular.

By isolated cottage, and built into hedge bank round garden.

Grey Granite (C.); 18 in. by 15 in. by 9 in.; sub.

Several similar Granites about 9 in. diam.

Coarse reddish Granite (Eskdale); 15 in. by 15 in.; square.

Andesite (E. L.); 18 in. by 9 in. by 9 in.; sub-angular.

Ibid. 21 in. by 18 in.; sub.

Syenite (B.); 18 in. diam.; rounded.

Also several other Andesites and Syenites of various sizes.

By gates, Springslade Lodge.

Grey Granite (C.); 18 in. by 15 in. by 12 in.; sub.

Colton (*opposite the gates to Bellamoore Hall*).

Felsite (E. L.); 18 in. diam.; well rounded but slightly broken.

On the bridge over stream.

Four Grey Granites (C.); varying in size from 30 in. by 24 in. by 18 in. to 42 in. by 30. in by 18 in.; sub.

In the village.

Grey Granite (C.); 30 in. by 18 in. by 18 in.; sub.

Also several smaller blocks of Grey Granite and a few Andesites.

Colwich (*in village*).

Grey Granite (C.); 12 in. diam.; rounded.

Pottal Reservoir (*along road at side*).

Rhyolite (crushed); 9 in. diam.

Grey Granite; 18 in. diam.

Andesite (E. L.); 36 in. by 24 in.; sub.

Fine grained Grey Granite; in ground; only upper surface exposed; 48 in. by 18 in.

Ibid. 12 in. diam.

Ibid. 18 in. by 15 in. by 15 in.

Coarse Red Granite (Eskdale); 21 in. by 18 in. by 15 in. flat, square angles.

Huntington (*at junction of roads by King's Arms*). [Level 550.]

Coarse Grit; 36 in. by 21 in. by 21 in.; sub.

The hedge banks are made up of boulders throughout whole length of village, amongst which are the following:—

Andesite (E. L.); 18 in. diameter; sub.

Felsite; 18 in. by 15 in. by 12 in.; sub.

Thirteen Andesites (E. L.); from 18 in. diam. up to 36 in. by 30 in. by 24 in., and consist of rounded, sub-angular, squarish, and irregular blocks.

Eight Grey Granites (C.); from 18 in. diam. up to 36 in. by 24 in., and consist of rounded and sub-angular blocks.

Local Sandstone; 12 in. by 9 in.; sub.

Felsite (buried in bank); 24 in. by 21 in. on face.

Two Syenites (B.); about 18 in. diam.; sub.

At corner of lane to the right, in bank.

Andesite (E. L.); 48 in. by 30 in.

Near same.

Grey Granite (C.); 24 in. by 24 in.

Andesite; 24 in. by 24 in. by 21 in.

Cannock (*near railway station*).

Andesite (E. L.); 36 in. by 30 in. by 30 in.; sub.

Syenite (E. L.); 30 in. by 24 in. by 24 in.; sub.

Near church.

Andesite (E. L.); 36 in. by 36 in. by 36 in.

On Wolverhampton Road, in bank.

Grey Granite (C.).

Bluish-grey coarse Granite (? Scotch); 12 in. diam

Syenite (B.); 15 in. by 12 in.; sub.

Felsite (E. L.); 15 in. by 9 in.; sub.

Ibid. 24 in. by 18 in.; sub.

Andesite; 30 in. by 24 in.

Coarse Red Granite (Eskdale); 18 in. by 12 in.

Road from Church Bridge to Hatherton, by stream.

Twelve Grey Granites (C.); from 18 in. to 24 in. diam.; sub-angular and rounded.

Eight Andesites (E.L.); about same size.

Beyond cross-roads.

Grey Granite (C.); 42 in. by 24 in.; irregular.

Near Hatherton.

Grey Granite (C.); 18 in.; angular.

Andesite (E.L.); 24 in. by 18 in.; smooth.

Quartz; 24 in. by 24 in.; square.

Near gates to Hatherton Hall

Three Granites (C.); from 15 in. to 29 in.; irregular.

Two Andesites (E.L.); about 24 in. by 18 in.; irregular and sub-angular.

One Andesite; 48 in. by 30 in.; irregular,

Syenite (B.); 18 in.; irregular.

Cheslyn Hay (*at cross-roads*). [Level 380.]

Grey Granite (C.); 36 in. by 36 in. by 30 in.

Four Crosses.

Five Grey Granites (C.); about 36 in. by 24 in. by 21 in.; sub. and rounded.

Andesite (E. L.); 33 in. by 18 in. by 18 in.

Also large number of similar rocks, about 12 in. diam.

Little Saredon. [Level 421.]

The village is situated on ground sloping towards S.W. Vast numbers of boulders occur in this village; nearly all the hedge banks are formed of them, and several houses and yard walls have boulders built into their foundations. Amongst them are the following:—

Lying against and forming foundations of farmyard walls.

[Level 433.]

Andesite (E. L.); 42 in. by 36 in.; sub.

Coarse Red Granite (Eskdale); small.

Syenite (B.); 15 in. by 12 in.; sub.

Grey Granite (C.); 30 in. in length on surface.

Coarse Hornblendic Granite (S. Scotland); 36 in. by 21 in. by 18 in.; squarish, broken.

Fine Grit; 36 in. by 36 in. by 27 in.; sub.

Grey Granite (C.); 36 in. by 30 in. by 24 in.; sub.
Ibid. Small; built into wall.
Syenite (B.); built into wall.
Grey Granite (C.); about 4 ft. long; built into wall.
Ibid. 21 in. by 21 in. by 15 in.; sub.
Ibid. 42 in. by 24 in. by 24 in.

Saredon (*lying at foot of garden wall to the Hall*). [Level 478.]
Syenite (B.); 30 in. by 24 in. by 15 in.; sub.
Grey Granite (C.); 30 in. by 24 in. by 24 in.; sub.
Ibid. 24 in. by 24 in. by 18 in.; sub.

Near same; at corner of lane to Little Saredon.
Grey Granite (C.); 36 in. by 36 in. by 18 in.; sub.

Down lane.
Grey Granite (C.); 48 in. by 36 in. by 24 in.; angular.
Ibid. 48 in. by 30 in. by 30 in.; angular.

Latherford (*in bend of lane*).
Grey Granite (C.); 42 in. by 30 in. by 24 in.; sub.

On bridge over stream.
Two Grey Granites (C.); 30 in. by 18 in. by 18 in.; oblong and angular.

By windmill.
Grey Granite (C.); 48 in. by 36 in. by 36 in.
Andesite (E. L.); 30 in. by 24 in. by 15 in.; sub.
Ibid. Small.
Grey Granite (C.); 21 in. by 18 in.; squarish.

Shareshill.

Immense numbers of boulders, as in last village, forming foundations of houses, walls, and hedges.

At corner of Bull's Head Public House. [Level 436.]
Andesite; about 30 in. diam., piece broken off.

Lower Lodge. [Level 448.]
Three Andesites (E. L.); about 24 in. by 24 in. by 18 in.; sub.
Felsite; 45 in. by 36 in. by 24 in. triangular.

Moseley (*Brook House*).

Six Felsites (E. L.); from 30 in. by 30 in. by 24 in. up to 60 in. by 36 in.; sub.

Several other Felsites, Andesites, Criffel Granites, etc.

By Northycote Farm. [Level 470·7.]

Andesite (E. L.); 30 in. by 24 in. by 18 in.; sub.

Grey Granite (C.); 24 in. by 24 in. by 18 in.; sub.

Ibid. 18 in. diam.; rounded.

Bushbury (*In spinny, on Bushbury Hill, near old gravel pit*). [Level 530.]

Coarse-grained Hornblendic Granite (S. Scotland); 42 in. by 24 in. by 18 in.; sub.

Four Andesites (E. L.); from 21 in. by 18 in. by 12 in. up to 48 in. by 30 in. by 21 in.; sub.

Ibid. With veins of quartz; 36 in. by 21 in. by 21 in.; crushed.

Fifteen Grey Granites (C.); from 18 in. by 18 in. up to 48 in. by 42 in. by 30 in.; and consist of angular, sub-angular, rounded, oblong, and squarish blocks.

Syenite (E. L.); 42 in. by 30 in.; sub.

Ibid. 36 in. by 24 in. by 18 in.; sub.

Grey Quartziferous Grit; 18 in. by 12 in. by 9 in.; angular.

Felsitic Ash; 42 in. by 36 in. by 24 in.; flattish and broken.

Syenite (B.); 18 in. by 12 in. by 12 in.; angular.

In lane near spinny.

Six Grey Granites (C.); from 24 in. by 18 in. up to 42 in. by 30 in. by 24 in.; sub.

In fields on south of hill.

Four Grey Granites (C.); from 30 in. by 18 in. by 18 in. to 36 in. by 24 in. by 18 in.; sub.

Local Sandstone; 18 in. by 15 in. by 9 in.; sub.

Felsite (E. L.); 30 in. by 24 in. by 18 in.; sub.

In lane northwards. [Level 589·8.]

Felsite (E. L.); 48 in. by 24 in. by 24 in.; oblong.

Andesite (E. L.); 24 in. by 18 in.

Granite (C.); 30 in. by 24 in.

In Bushbury village. [Level 447.]

Boulders in large numbers; majority consisting of Criffel Granites. Amongst them are :—

Five Grey Granites (C.); from 42 in. by 30 in. by 24 in., up to 60 in. by 30 in.

Andesite (E. L.); 36 in. by 36 in. by 18 in.; squarish.

Syenite (E. L.); 30 in. diam.; rounded, smooth.

The last three or four are situate under a large cedar tree, the roots of which have grown over them.

Essington (*Woodhays*).

Andesite (E. L.); 36 in. by 24 in. by 18 in.; sub.

Oakley Farm. [Level 500.]

Syenite (B.); 30 in. diam.; rounded.

Between this and Blackhalve Farm.

Two Andesites (E. L.); 42 in. by 24 in.; sub.

Two Grey Granites (C.); 24 in. by 18 in. by 15 in.; sub.

Two Andesites (E. L.); 30 in. diam.; rounded.

Near Minerva Inn. [Level 600.]

Grey Granite (C.); 42 in. by 30 in. by 24 in.; sub.

Also a large number of small blocks, consisting of Andesites, Syenites, Granites, etc.

In lane towards church.

Grey Granite (C.); 42 in. by 18 in. by 18 in.; sub.

Eight Andesites (E. L.); about 30 in. diam.; roughly rounded.

Felsite (E. L.); 30 in. by 24 in. by 24 in.; rounded.

Quartzite; water-worn and rounded.

Codsall to Brewood (*Bilbrook*).

Five Granites (C.); about 42 in. by 30 in. by 24 in.; sub.

Felsite; 24 in. by 18 in.

Long Birch Farm.

Andesite (E. L.); 30 in. by 24 in. by 24 in.; sub.

Hornblendic Granite (S. Scotland); small.

Granite (C.); 42 in. by 42 in. by 30 in.; flat-sided.

Granite (Eskdale); small.

Syenite (B.); small.

Giffard's Cross.

Andesite (E. L.); 24 in. by 24 in. by 18 in.; sub.

Brewood.

Andesite (E. L.); 36 in. by 36 in. by 24 in.; flat and broken.

Ibid. Small.

Syenite (B.); small.

Pendeford

Syenite (E. L.); 42 in. by 30 in. by 24 in.; angular.

Ibid. 18 in. by 12 in.

Ibid. 30 in. by 24 in.

Granite; 24 in. by 24 in.

Ibid (C.) 30 in. by 18 in.

Coven.

Large numbers of boulders of all kinds built into walls and hedge foundations, of all sizes.

Trysull.

Large deposit of boulders; the garden walls and hedge banks are mostly built of them. Amongst others:—

Felsite; about 30 in. diam.

Two Granites (C.); 30 in. by 30 in. by 24 in.; squarish.

Andesite (E. L.); 18 in. diam.

Ibid. 30 in. by 24 in. by 18 in.

Ibid. 36 in. by 30 in.

Syenite (B.); 30 in. by 24 in.

Coven Lawn.

Felsite; 30 in. by 24 in.

In lane towards Lock House, and in hedge bank.

Hornblendic Granite (S. Scotland); 54 in. by 36 in; squarish.

Ibid. 18 in. by 18 in.; ditto.

Ibid. 36 in. by 30 in.; sub.

Two Granites (C.); about 36 in. by 24 in.

Andesite (E. L.); 30 in. by 24 in.; sub.

Occupation road over canal.

Granite (C.); 30 in. diam.

Road to Oreton, past lock gate.

Andesite; 18 in. diam.

Oreton.

Wall built of small boulders of the several kinds.

On the hill, opposite farm.

Andesite; 18 in. diam.
Granite (C.); 18 in. diam.
Three Andesites (E. L.); 30 in. by 24 in.; sub.
Coarse Red Granite (Eskdale); 24 in. by 24 in. by 18 in.
Granite (C.); several about 24 in. by 24 in.
Felsite (E. L.); 30 in. by 24 in.
Syenite; 30 in. by 24 in. by 24 in.; oblong.
Andesite (E. L.); 30 in. by 30 in.; flat faces.

Junction with turnpike road.

Granite (C.); 15 in. diam.

Penn (*opposite Hollybush Inn*).

Andesite (E. L.); 30 in. by 24 in; oblong.

Penn Villa.

Granite (C.); 30 in. by 24 in.; broken.
Syenite (B.); 9 in. diam.
Five Andesites (E. L.); about 36 in. by 30 in. by 24 in. angular.
Hornblendic Granite; several about 9 in. diam.
Granite (C.); 30 in. by 24 in.; sub.
Andesite; 24 in. by 18 in.

Opposite gate.

Granite (C.); 36 in. by 24 in.
Syenite; 36 in. by 24 in.

Boningale.

Two Granites (C.); 30 in. by 24 in. by 18 in.; sub.
Syenite (B.); 21 in. by 18 in. by 15 in.

Pepper Hill.

Granite (C.); 30 in. by 24 in.
Ibid. 18 in. diam.

Oaken (*at road corners*).

Two Granites; 24 in. diam.

Shifnal to Sutton Maddock.

The Wyke.

Several Andesites and Syenites varying from 18 in. by 12 in. to 12 in. by 9 in.; more or less rounded and sub-angular.

The Hen Mill.

Fine-grained Hornblendic Syenite; 18 in. by 15 in.; sub.

Andesite (E. L.); 18 in. diam.

By cross-road to Madeley.

Coarse Granite (Eskdale); small.

Andesite (E. L.); 30 in. by 24 in. by 18 in.; roughly squared.

Ibid. 24 in. by 15 in. by 15 in.; slightly smoothed.

Ibid. 18 in. by 15 in.

Near Brockton Hall.

Andesite (E. L.); 36 in. by 30 in, by 21 in.; sub.

Granite (C.); 36 in. by 27 in. by 15 in.; flattish sides.

Ibid. 24 in. by 18 in.; triangular.

Andesite (E. L.); 24 in. by 18 in.; rounded.

Ibid. 12 in. diam.

Ibid. 24 in. diam.

Syenite (E. L.); 30 in. by 24 in. by 18 in.; broken.

Just beyond cross-roads.

Granite (Eskdale); 24 in. by 18 in. by 15 in.

Andesite (E. L.); 30 in. by 24 in.

Syenite (E. L.); 24 in. by 18 in.

Granite (C.); 18 in. by 12 in.

On footpath to Sutton Maddock.

Blue Shale, 30 in. by 30 in. by 24 in.; smooth, squarish.

Andesite (E. L.); 24 in. by 15 in. by 12 in.; smooth, squarish.

Cross-roads beyond church

Granite (C.); 24 in, by 24 in. by 18 in.

Two Syenites (E.L.); 24 in. by 18 in. by 18 in.

By New House.

Granite (C.); 36 in. by 30 in. by 24 in.; sub.
Syenite (B.); 24 in. by 18 in. by 9 in.; sub.
Ibid. 18 in. by 15 in.; sub.
On stone heaps large quantity broken pieces of coarse and fine Eskdale Granite, Andesite, and Syenite.

HARRINGTON.

Granite (C.); 36 in. by 30 in. by 24 in.; sub.

GRINDLE.

Granite (C.); 18 in. by 18 in. by 12 in.; sub.
Coarse Hornblendic Granite; 30 in. by 24 in. by 24 in.; sub.

HIGHGATE, NEAR ENVILLE (*on the common*).

Granite (C.); 18 in. by 15 in. by 12 in.; sub.
Felsite; 33 in. by 24 in. by 24 in.; sub.

In lane.

Granite (C.); 21 in. by 18 in. by 9 in.
Syenite (E. L.); 15 in. by 12 in. by 9 in.; sub.

By Mere Farm.

Felsite (E. L.); 21 in. by 18 in. by 18 in.; square.
Coarse Granite (Eskdale); small, angular.
Felsite (E. L.); 21 in. by 18 in. by 18 in.; sub.
Bluish Grey Watery Quartz Granite; 21 in. by 18 in. by 18 in.; sub.
Coarse Granite (Eskdale); 30 in. by 24 in. by 15 in; flat sides, rounded margins.
Blue Grit; 18 in. by 9 in.
Andesite (E. L.); 18 in. by 18 in. by 12 in.
Ibid. 30 in. by 18 in. by 15 in.; sub.
Ibid. 18 in. by 12 in.

Margin of pool.

Three Granites (C.); about 18 in. diam.
Andesite.
Local Sandstone; 36 in. on face.

Coarse Granite (Eskdale); 24 in. by 21 in. by 18 in.; square.
Three *Ibid.* 18 in. by 18 in.; sub.
Andesite (E. L.); 18 in. by 18 in.
Ibid. 24 in. diam.; rounded.
Ibid. 36 in. diam.; rounded.
Four Felsites (E. L.); about 30 in. by 24 in.
Granite (C.); 42 in. by 30 in. by 24 in.; sub.
Ibid. 30 in. by 30 in. by 24 in.; sub.

By junction lane to Lutley.

Syenite (B.); 15 in. by 12 in. by 9 in.; sub.
Felsite (E. L.); 15 in. by 12 in. by 9 in.; sub.
Ibid. 27 in. by 24 in. by 18 in.; sub.
Andesite (E. L.); 21 in. by 18 in. by 15 in.; angular.
Granite (C.); 30 in. by 18 in. by 15 in.; sub.

By bridge over stream.

Andesite (E. L.); 36 in. by 30 in. by 24 in.; squarish.

Morfe Farm.

Large numbers of similar rocks to the above.

Much Wenlock to Burton.

Granite (C.); 18 in. by 9 in.; sub.
Five Andesites (E. L.); 42 in. by 30 in. by 24 in.
Ibid. 36 in. by 24 in. by 18 in.
Ibid. Several about 24 in. by 18 in. by 18 in.
Granite (C.); 21 in. by 15 in. by 15 in.; oblong.
Coarse Red Granite (Eskdale); about 12 in. diam.
Bluish Grey Grit; 18 in. diam.
Three Andesites (E. L.); 21 in. diam.
Coarse Red Granite (Eskdale); 18 in. by 9 in.; sub.
Two Andesites (E. L.); 18 in. by 18 in.
Large number of Eskdale Granite in small blocks on stone heaps, from 6 in. to 9 in. diameter.

Burton (*near farm*).

Felstone (Welsh?); 30 in. by 24 in. by 15 in.
Coarse Granite (Eskdale); 30 in. by 24 in. by 21 in.

Junction of lanes.

Andesite, very much crushed; 18 in. by 12 in. by 9 in.
Granite (C.); 18 in. by 15 in. by 12 in.; sub.
Ibid. 42 in. by 30 in. by 15 in.; oblong.
Ibid. 24 in. by 18 in. by 12 in.; oblong.
Ibid. 24 in. by 15 in. by 12 in.; oblong.
Fine-grained Granite (C.); 24 in. by 15 in. by 12 in.; sub.
Six Andesites (E. L.); 24 in. by 24 in. by 15 in.
Granite (Eskdale); 12 in. by 9 in. by 9 in.; sub.
Andesite (E. L.); 36 in. by 18 in. by 15 in.; irregular.
Grey Granite (C.); 15 in. by 12 in. by 9 in.; sub.

At meeting of four roads.

Grey Granite (C.); 30 in. by 24 in. by 24 in.; sub.

Near same.

Andesite (E. L.); 36 in. by 24 in. by 18 in.

Burton to Westwood Barn.

Felsite (Arenig ?); 48 in. by 36 in. by 24 in.; broken; was probably once 60 in. by 48 in. by 24 in.
Granite (Eskdale); 24 in. by 18 in. by 18 in.; sub., smooth.
Ibid. 18 in. by 12 in. by 12 in.
Seven Felsites (E. L.); from 21 in. by 21 in. by 18 in. to 30 in. by 24 in. by 24 in.; sub.
Two Granites (Eskdale); 24 in. by 24 in. by 18 in. rounded.

At end of lane, by gate.

Felsite (E. L.); 36 in. by 24 in. by 24 in.; sub., smooth.

In hedge, just beyond last.

Granite (C.); 30 in. diam.; rounded.
Ibid. (Eskdale); 24 in. by 18 in. by 18 in.; oblong.
Two Felsites (E. L.); 30 in. by 30 in. by 21 in.; sub.
Andesite (E. L.); 42 in. by 24 in. by 24 in.; oblong.
Hornblendic Granite; 48 in. by 24 in. by 24 in.; angular and oblong.

Through gate.

Andesite (E. L.); 60 in. by 36 in. by 24 in.; broken; must have once been 6 ft. or 7 ft. long; sub., smooth.

Ibid. 36 in. by 24 in.

Burton to Patton.

Andesite; 18 in. by 18 in. by 15 in.

Ibid. 18 in. diam.

Granite (C.); 9 in. diam.

Ibid. 12 in. diam.

Andesite; 18 in. diam.

Burton to Monkley (*near hut*).

Andesite (E. L.); 42 in. by 36 in. by 24 in.; flattish.

Granite (C.); 48 in. by 36 in. by 30 in.; oblong, uneven surface.

Ibid. 36 in. by 30 in. by 18 in; sides flat, squarish.

By Brickworks.

Ibid. 15 in. diam.

Andesite (E. L.); 18 in. by 15 in.; sub., smooth.

Ibid. 24 in. by 24 in. by 18 in.; angular.

Beggar Hill Bank.

Ibid. 18 in. by 12 in. by 9 in.; sub., smooth.

Granite (C.); 12 in. diam.; rounded.

Ibid. 9 in. diam.; rounded.

In hollow.

Ibid. 15 in. by 12 in. by 9 in.; sub.

Reddish Hornblendic Granite; 18 in. by 12 in. by 9 in.; sub.

Near the Marsh.

Granite (C.); 30 in. by 24 in. by 18 in.; sub., all angles, well rounded, and smoothed.

Andesite; 24 in. by 18 in. by 21 in.

Felstone; 36 in. by 36 in. by 24 in.; rough.

On Wenlock Edge (*opposite Longville Station*).

Five Felstones; from 18 in. by 18 in. by 12 in. up to 48 in. by 42 in. by 36 in.; well rounded angles, and faces hollowed out into cavities.

At foot of hill.

Ibid. 30 in. by 24 in. by 18 in.

By stream, below Wenlock Abbey.

Grey Granite (C.); 36 in. by 30 in. by 24 in.; sub., smoothed, hollowed, and angles rounded off.

Droitwich *(road to Hartlebury, just beyond brook).*

Andesite (E. L.); 18 in. diam.; smooth.

Ibid. 15 in. by 12 in. by 12 in.; sub.

Ibid. 24 in. by 15 in. by 15 in.; sub.

Felsite (E. L.); 12 in. by 9 in. by 9 in.; sub.

Syenite; 15 in. by 12 in. by 9 in.; sub.

Granite (C.); 18 in. by 12 in. by 12 in.

Churchill *(in lane, by Church).*

Felsite (Arenig); 36 in. by 30 in.; rough.

In lane, north of village.

Ibid. 84 in. by 48 in. by 48 in.

Hagley *(in lane from Railway Station to Hagley Brake).*

Felsite (Arenig); 42 in. by 21 in.; out of ground, squarish.

Two *Ibid.* About 18 in. by 18 in.

Corner of lane to Clent Grove.

Felsite (Arenig); 48 in. by 24 in. by 15 in.; square angles.

Clent.

Ibid. 36 in. by 24 in. by 18 in.; triangular, sharp angles.

Bellbroughton to Barnt Green.

Madeley.

Felsite (Arenig); 54 in. by 30 in. by 24 in.; square and rough.

Ibid. 48 in. by 36 in. by 24 in.; square angles, but slightly wedge-shaped.

Ibid. 36 in. by 24 in. by 18 in.; square, rough.

Ibid. 24 in. by 18 in. by 15 in.; triangular.

Ibid. 48 in. by 36 in. by 30 in.; triangular.

Ibid. 54 in. by 30 in. by 24 in.; irregular, rough.

At cross-roads.

Felsite (Arenig); 48 in. by 30 in. by 24 in.; out of ground.

Calcot.

Block of Quartz in hedge bank; 36 in by 36 in. by 24 in.

Romsley Hill. [Level 900.]

Felsite (Arenig); about 48 in. by 30 in. by 24 in.

Basalt (Local); about same size.

Dayhouse Bank.

Felsite (Arenig); 36 in. by 24 in. by 21 in.

Sandstone (*Gannow Mill*).

Large number of Felsites (Arenig) and Permian Breccia; in size, about 48 in. by 36 in. by 24 in.; used in hedge banks and walls.

Gannow Green.

Carboniferous Sandstone; 36 in. by 18 in. by 18 in.

Ibid. 30. in by 24 in. by 15 in.

Felsite (Arenig); about same size.

Rubery (*by New Inn*).

Several Felsites.

Carboniferous Sandstone; 30 in. by 27 in. by 24 in.

By spring. [Level 684.]

Felsite (Arenig); 114 in. by 48 in. by 36 in. square angles, rough.

Ibid. 60 in. by 48 in. by 36 in.

Ibid. 18 in. by 18 in.

By farm.

Ibid. 36 in. by 30 in. by 15 in.; square angles.

Ibid. 36 in. by 15 in.; square angles.

Ibid. 24 in. by 24 in.; rounded.

Near Cock Inn.

Ibid. 36 in. by 36 in. by 24 in.

Ibid. 48 in. by 30 in. by 24 in.

Ibid. 36 in. by 24 in. by 18 in.

Farm near Railway Station.

Ibid. About same size.

Ibid. 24 in. by 24 in. by 18 in.

Sandstone ; 48 in. by 24 in. by 18 in.

Near Station.

Felsite ; 60 in. by 30 in.

Ibid. 48 in. by 48 in.

Road to Northfield, near farm.

Felsites (Arenig) ; two, about 30 in. by 24 in. by 18 in.

Near Tinker's Farm.

Ibid. 42 in. by 42 in. by 24 in.

Ibid. 48 in. by 27 in. by 24 in.

Northfield (*in lane near Church*).

Ibid. 48 in. by 36 in.

Ibid. 30 in. by 24 in.

Ibid. 42 in. by 30 in. by 24 in.

Ibid. "The Great Stone ;" 60 in. by 27 in. by about 30 in.

Several other blocks of same kind and varying from 30 in. to 48 in. diam.

Footpath by Church.

Felsite ; 36 in. by 24 in.

Ibid. 54 in. by 30 in. by 24 in.

Ibid. 60 in. by 36 in.

And several more of large size.

Besides the above, there are numbers of blocks of slate, and shale and conglomerate.

In lane, back of Lucy's Mill.

Several Felsitic blocks, varying from 18 in. to 36 in. diam.

Burcot (*at bottom of lane from Barnt Green*).

Felsite (Arenig) ; 42 in. by 15 in.

Ibid. 66 in. by 36 in. by 24 in.

Bromsgrove *(Battlefield Hill).*

Felsite (Arenig) ; 24 in. by 18 in. ; triangular.

Half-way House, by gates.

Felsite (Arenig); 42 in. by 24 in. by 18 in.; triangular, flattish.

Ibid. 36 in. by 18 in. by 18 in.; oblong.

Ibid. 24 in. by 24 in. by 18 in.

Ibid. 30 in. by 24 in. by 18 in.

Ibid. 24 in. by 18 in. by 18 in.

Woodcote.

Felsite (Arenig); 30 in. by 24 in. by 18 in.; flattish.

Ibid. 54 in. by 30 in.; flattish.

Ibid. 30 in. by 24 in. by 18 in.; flattish.

Ibid. 42 in. by 30 in. by 18 in.; flattish.

Finstal (*in lane from Bromsgrove Railway Station*).

Felsite (Arenig); 21 in. by 18 in. by 18 in.; oblong.

Ibid. 33 in. by 24 in. by 21 in.; oblong.

Ibid. 18 in. by 12 in. by 12 in.; sub.

Lane by Spadesbourne Brook.

Felsite (Arenig).

Basalt.

In High Street.

Felsites (two); about 42 in. by 30 in.; and several smaller ones.

Sandstone.

Lane to Baddington Mill.

Felsite (Arenig); 60 in. by 60 in. by 30 in.

Three smaller ditto.

King's Norton, Stirchley, Harborne, Selly Oak.

Large numbers of boulders of Welsh origin occur in these districts, of which mention has already been made in the Proceedings of the Society.

Soho.

Several boulders of Welsh origin have also been found here, both at Icknield Street and in the Perry Barr railway cutting, and have been recorded.

Aldridge Road, just beyond Perry Barr Station.

Felsite; well smoothed; 30 in. by 18 in. by 18 in.

Gravelly Hill (*in brickyard*).

Felsite (Arenig); 24 in. by 15 in. by 15 in.

Erdington (*clay pits, near Station*).

Felsite (Arenig); 30 in. by 15 in. by 12 in.

Ibid (two); small blocks.

Andesite (E.L.); small.

Bluish Grey Granite (S. Scotland); small angular.

Mudstones; small.

Sutton Coldfield (*in road, back of Royal Hotel*).

Granite (Criffel); about 12 in. diam.

Andesite (E.L.); 24 in. by 15 in.

In Park, near Mayor's Cottage.

Granite (Criffel); 12 in. diam.

Ibid. In railway cutting; 15 in. by 12 in.; square.

In quarry, near Lichfield Road.

Several small blocks, including:—

Felsite (Arenig).

Felsite (E.L.)

Granite (Criffel).

Syenite (Buttermere)

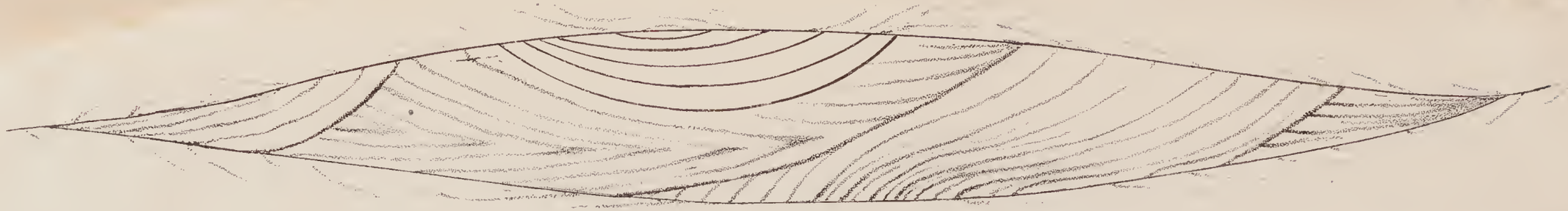

Fig.3. *Buff Lenticle shewing markings, with rose red sandstone above and below. Length 14 ft.; greatest width 2 ft.*

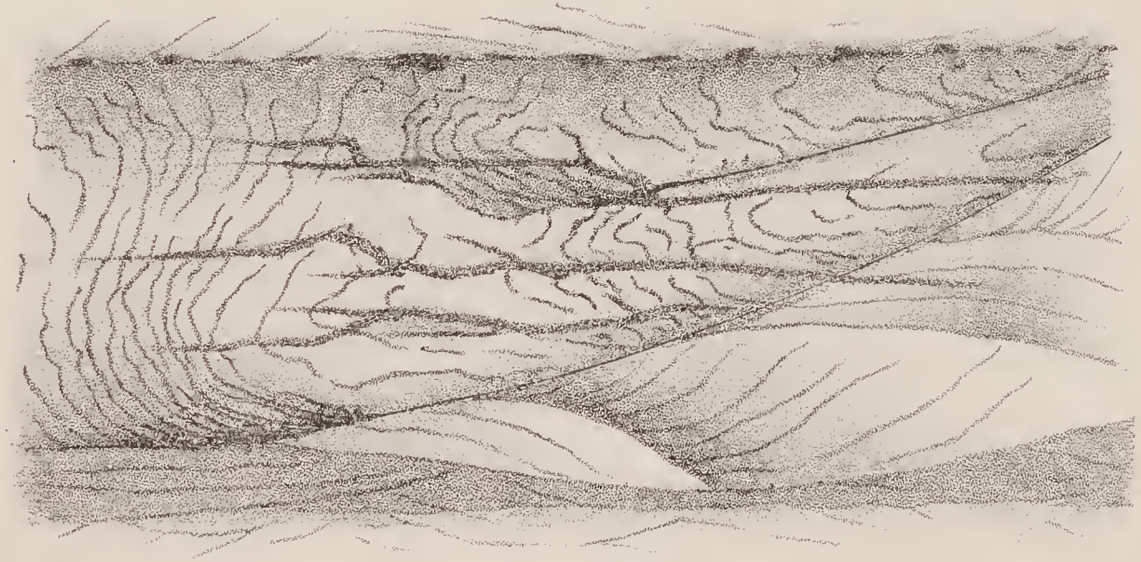

Fig.1. *Portion of bed No. 5 shewing markings.*

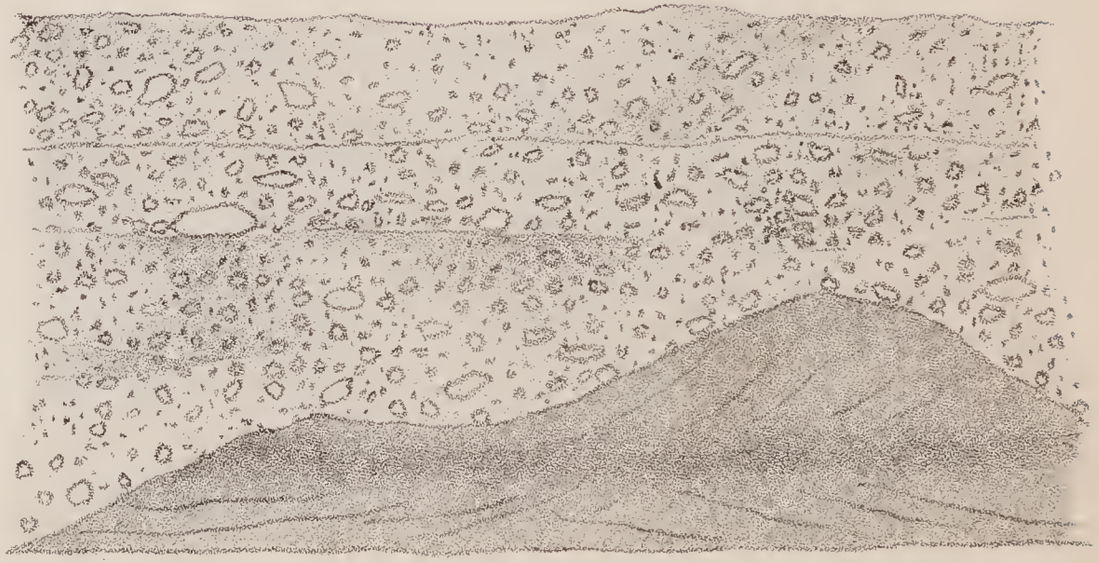

Fig. 2. *Unconformity of Sandstone and Conglomerate.*

J. Landon. del.

HERALD PRESS LITH BIRMM

Fig.4. *Portion of bed No. 10 shewing current bedding. Length about 20 ft.*

VIII.—*The Barr Beacon Beds.*

By Joseph Landon, F.G.S.,

Senior Lecturer in Saltley Training College.

[Read before the Geological Section, June 5th, 1890.]

Passing from Birmingham in a direction slightly W. of North, we find the land rising in successive undulations with intervening valleys through Handsworth, Hamstead Hill, and Great Barr, until, at a distance as the crow flies of nearly 7 miles, the culminating point is reached in Barr Beacon. The height of this hill is about 800 feet, being about 300 feet above that of Handsworth.

Standing on the top of the Beacon the topography of the country is easily made out for some distance. N. and S. runs a distinct ridge for some $3\frac{1}{2}$ miles, ending to the S. near Barr Mill, and at its northern extremity sweeping round to the eastward. The Beacon stands at about the centre of the ridge; and to the west, the land slopes rapidly all along into the valley, to rise again more gently in a second and much lower ridge. This is succeeded by another valley, and then comes a broken band of high-land still further west. To the N.N.E. lies the broad depression of Bourn Vale, to which the Beacon ridge forms the western and northern boundary. Eastwards and

south-eastwards the land descends in several flattening ridges, which run out in broad tongues—the northern ones towards Sutton Park. The most conspicuous and important of these lines of elevation is the one which bears on its broad top King's Standing Wood, then runs eastward through Welshman's Hill and past Beggar's Bush. To the west, this irregular belt of high ground drops in lateral ridges into the Queslett Valley, which descends somewhat sharply almost due south from the Beacon, with the Great Barr portion of the N. and S. ridge on its western side.

The district has been mapped by the Geological Survey as follows:—The whole of the N. and S. ridge of the Beacon and all the high grounds and depressions to the east are given as the Bunter Conglomerate—that is, the Pebble Beds which form the middle member of the Bunter series—as far as the eastern confines of Sutton Park in one direction, and as far south as Witton in another. To the west of the Beacon ridge the Pebble Beds are carried down to the bottom of the valley; and these are succeeded by a strip of Permian strata forming the high land on the other side of the depression. This strip narrows as it proceeds northwards, and at length disappears. Beyond the Permian is an irregular patch of Silurian rocks, extending in its widest development to Walsall, with the two Wenlock limestones coming to the surface near the town, and the lower Woolhope or Barr limestone cropping out at Hay Head near the eastern boundary of the formation. The Silurian rocks, and further south the Permian beds, are followed by the Coal Measures of the South Staffordshire Coalfield.

In his "Geology of the Warwickshire Coalfield," Mr. H. H. Howell, speaking of the district east of the South Staffordshire Coalfield, says: "It will be observed that the Lower Red and Mottled Sandstone, which is almost invariably present beneath the Conglomerate when the New Red Sandstone series is complete in all its divisions, is here absent." And further: "At Barr Beacon and Sutton Park we are enabled to estimate with tolerable accuracy the thickness which the Conglomerate attains, for here

we get nearly a complete section of it. The thickness of the Conglomerate according to this section is about 450 feet, and it continues about the same thickness further north, near Lichfield." In his explanation of the Horizontal Section through the district on Sheet 49, he also says: "At Barr Beacon the Conglomerate attains a thickness of *350 feet, and rests directly on Permian Strata.* The boundary between these Strata is about a quarter-of-a-mile W. of Barr Beacon. . . . . The Conglomerate rests uncomformably on the underlying Permian Strata, consequently the thickness of this latter formation cannot be accurately ascertained. Proceeding east along the line of section the Pebble Beds continue for a distance of 3 miles, having a dip eastward of about 3°. The fall of the ground having much the same inclination, the Conglomerate probably continues about the same average thickness as at Barr Beacon as far as the east side of Sutton Park. At this point the section crosses a fault, bringing the upper division of the Bunter or Soft Red and Mottled Sandstone against the Pebble Beds. The evidence of this fault is not clear at the point where the section crosses, but its existence is inferred from the fact of the overlying Keuper Sandstone being brought very near to the Conglomerate. The Upper Red and Mottled Sandstone is about 250 feet thick, and dips towards the west at an angle of 2°."

Some two years ago I made some rapid traverses of the district, on the advice of Professor Lapworth. Since then I have been over the ground many times, and have examined most of it carefully in detail. From what I saw of the nature of the beds occasionally exposed in small sections, and in the large section under the Beacon Hill, I came to the conclusion that there must be a considerable thickness of beds, almost, if not quite, devoid of pebbles between the Conglomerate and the Permian Strata. The Pebble Beds are well known, and the Permian Rocks of the district are so well exposed in several places that it is not difficult to make out pretty thoroughly their nature and sequence. The complete proof, however, of the

existence of the *intermediate sandstones* referred to above, and the determination of the exact nature of them, I was unable to accomplish until recently; when my friend, Mr. Sherwood drew my attention to the fact that a deep cutting was being made, about half-a-mile E. by S. E. of the Beacon trees, by the Gatling Gun Ammunition Co., for the purpose of erecting targets for the trial of shells.

The detailed study of this section, and a reinvestigation of the area under consideration, have afforded what appears to me incontestable evidence of the existence of the Lower Mottled Sandstone—which was said by the Survey to be absent here—and have thrown light upon many other points in connection with the geology of the district

The Lower Mottled Sandstone.—The existence of this on the east of the South Staffordshire Coalfield would doubtless have been made clear before, but for the fact that the surfaces of the slopes and valleys where it exists are covered with a drift-wash of the pebbles from the higher grounds, and occasionally no doubt by true drift gravels. The absence of sufficient exposures has also been a difficulty, and it is but fair to Mr. Howell, and the other officers of the Survey who mapped the district, to remember that probably few, if any, of the sections now exposed were open at the time the Survey was carried out.

The cutting referred to above is a long rectangular trench, the bottom being about 120 yards long by 7 yards wide. It is made in the slope descending from Barr Beacon towards Sutton Park, and is thus in the direct line of dip of the Pebble Beds. When I visited it first the sandstone had been excavated in a sheer wall at the higher end to a depth of some 22 feet, and work was also being carried on in the middle and at the lower end of the cutting. I have made careful drawings and notes of the whole, as the work has been carried through, resulting in a complete section from one end to the other of some 30 feet in depth. At the upper or deeper end of the cutting the Pebble Beds were found to be completely removed, nothing coming above the sandstone but some 18 inches of

pebbly soil; but westwards up the slope they appeared to come on almost at once, and at the lower end of the cutting the sandstones might be seen clearly passing beneath them. *(See fig. 2.)*

The section in detail, in an east and west direction, shown at the higher end of the cutting, with the lower beds added from another parallel and perpendicular section in the working near its lower end, is given below. The numbers mark the thicker beds, in descending order, distinctly separable at a little distance, the division planes being nearly parallel and almost horizontal.

1.—Deep rose red sandstone, in places dull crimson, with lighter red and brownish grey near the bottom. About the middle the bed was crossed horizontally by a six-inch belt of finer sandstone, probably with some admixture of clay. 3 feet.

2.—Light reddish grey sandstone, generally coarse, streaked and blotched with yellow and red, and with a few small patches of almost colourless grey. The lowest 6 inches of the bed formed part of a rich reddish brown or walnut-coloured strip or lenticle. 2 feet 6 inches.

3.—Light buff and brownish grey band, with patches of greenish yellow, red, and dull maroon. 2 feet 9 inches.

4.—Bed mostly light brown grey, coarse in texture; in places exceedingly light, with an irregular walnut-coloured band near the top. The bedding plane at the bottom was marked by a straight line of scattered fragments of purple clay, rarely more than an inch across. 2 feet 6 inches.

5.—Bed mostly light brown, passing into greenish ochre, buff, and light red, with a deep red band near the base. Cloudy streaks and patches occurred of dark brown, grey, and one or two of lemon yellow. This band was usually coarse in texture, intensely false-bedded, and crossed by hundreds of wavy lines. Reference will be made to it later on. 4 feet. *(See fig. 1.)*

6.—Brownish red sandstone, fairly hard, blotched, and in places passing into light grey, streaked with darker tints. 1 foot 3 inches.

7.—Deep brown red sandstone, with dull crimson patches, and mottled with small spots and streaks of yellow and greenish grey; clay fragments present. 3 feet 6 inches.

8.—Dark dull red band, with small lenticles of light greyish red, and with two oblique bands of about 6 inches each—one light buff, and one pinkish red—running into the base of the bed. 2 feet.

9.—Harder brownish purple sandstone, with a few small green spots. This band was very fine in texture, and was, perhaps, the most distinct of the whole. It did not alter its character in the least within the limits exposed, and there was no trace of false bedding. The whole lay in thin, straight, and perfectly parallel laminæ, about $\frac{1}{8}$ of an inch in width, which gave it the appearance of a hard sandy shale. 2 feet.

10.—Broad bed, made up for the most part of dark walnut-coloured sand, with bands and patches of red and grey. The whole was bedded in a very peculiar manner, and in places contained numerous brownish purple clay fragments from $\frac{1}{4}$ of an inch to 6 inches across, with occasional irregular strips of the same material. 4 feet 6 inches. *(See fig. 4.)*

This was underlain by a dull red, fairly hard, sandstone, but the limits of the bed could not be made out exactly.

The beds may be generally described as *soft sandstones*, sufficiently compact to form a firm perpendicular surface, with a few harder beds here and there. In many cases they are soft enough to be cut with a spade, but they harden on exposure, and are never loose. The *sand is of varying degrees of coarseness*, sometimes being rough, clean, and gritty to the touch; but occasionally very fine, with apparently a small admixture of clayey material, so that when struck with the hammer the surface shines as though the sandstone had a marly texture. Throughout the series, so far as exposed, there is an *entire absence of pebbles. Fragments of purple clay are present in several of the beds*, especially the one marked No. 10. *(See fig. 4.)* These are often an inch or two across, with a few larger blocks and the unbroken surfaces are in nearly all cases black.

There can scarcely be a doubt that these fragments of clay have been derived from the Permian clays farther west. The *sands are exceedingly bright in colour when freshly exposed*, showing a great variety of reds, from pink to chocolate and deep reddish brown, with yellows, greys, purples, and light grey greens; and are blotched, clouded, streaked, and spotted in a truly wonderful way. That I have exaggerated neither the colours nor the brightness of the tints when freshly exposed, my friends, Mr. Udall and Mr. Sherwood, both of whom have seen the beds, are able to testify. On exposure to the atmospheric influences, the colours seem somewhat rapidly to fade into duller tints of red, brownish red, and grey; and the general tint of the great bank of sand which has been taken out of the cutting is distinctly light red. The colouring of the beds, again, is by no means constant; a light bed sometimes passing into deeper red, and a dark one into a grey. *All the beds with the exception of No. 9 are very strongly marked by the peculiar waving and fanning lines known as "false bedding,"* some excessively so. In fact, the bed marked No. 5 is by far the most striking instance of this which has ever come within my experience. In the space of a few feet, hundreds of reddish brown lines run irregularly downwards across the bed, sweep round in approaching curves until they come to a point on the oblique lines bounding one of the larger lenticles, and then fan out again in some other direction, it may be in flatter curves, in the most complex way. (*See fig. 1.*) Another peculiar characteristic of many of the beds, especially the greyer ones, is that, in addition to the false bedding lines, and quite distinct from them, bright red lines, from the thinnest trace up to about 2 inches wide, run in a broken and irregular manner roughly parallel to the larger bedding planes. These fade out or end suddenly, and are joined by cross lines until sometimes they form a kind of strange network over the perpendicular surfaces of the beds, as though the rock had been painted with a brush. Viewing the long sides of the cutting, the beds are seen to be *made up in almost all cases of long lenticles*, or double

wedges, usually from 8 to 20 feet long, and from 1 to over 2 feet wide. This current bedding often causes the beds to vary a little in thickness and sometimes obscures the larger bedding planes almost completely. The general parallelism of the beds, however, is not difficult to make out, and the bed No. 5 was traced without any appreciable variation in thickness for about 34 yards. The lenticular formation seems almost certainly due to the action of currents, as not only does the structure appear to indicate this, but whenever lines of clay occur, they almost always lie on the junction surface of two such lenticles. The smaller peculiarities of marking—the waved and puckered lines *(see fig. 1)*—may be the result in part of disturbance by these currents without rearrangement, and in other instances may be due to small eddies. In fact, much of the so-called false bedding I have observed appears to me to be more reasonably explained as the effect of gradual deposition in minor eddies and swirls than in any other way.

At first sight the beds at the deeper end of the cutting appeared almost horizontal. Careful examination, however, shewed there was a slight tilt to the east. The clearly defined and perfectly straight-edged bed No. 9, which crossed the cutting near the lower end from west to east, and lay quite parallel to the main bedding planes, afforded the best means of determining the amount of the depression, and shewed a *distinct dip to the east of about 3°*.

The relation of the sandstones to the Pebble Beds is perfectly clear. The fact that the higher lands all round the section are distinctly composed of conglomerate, shews that the sandstones lie beneath them; but other evidence is not wanting. Near the lower end of the cutting a passage or road has been made at a depth of 4 or 5 feet, and here the undulating and clearly eroded surface of the sandstone may be seen in several places actually lying beneath some 2 feet or so of compact conglomerate, the bedding of which is distinct, with another 18 inches of disturbed pebbles or surface wash over this. *(See fig. 2.)*

Passing from the cutting to Barr Beacon by the road to the south, several insignificant exposures of red and buff sand occur as we approach the cross roads just S. of the Beacon. From this point towards Great Barr similar small exposures may be found on the steep tree-covered western slope of the N. and S. ridge; and at the bottom of the valley the stream banks show light red and grey sands, clearly not derived from Permian Strata. Returning over the ridge into the valley leading down to Queslett, the bright red sand is frequently turned up by the plough, and exposed in many places in the banks beneath the hedges. There can be little doubt that the whole of the lower parts of this valley are hollowed out of the Lower Red and Mottled Sandstone, and are only covered with a superficial wash of pebbles from the higher land.

Near Half-way House Farm, about a mile and a half S. of Queslett, a well-marked ridge runs N.E. and S.W. This may possibly have a capping of Pebble Beds, or of ordinary drift, but, from numerous rabbit burrows on the slopes, light red and buff sand has been thrown out, exactly similar to that of the described beds after exposure, and quite unlike the Permian conglomerates and dark brown sandstones which here lie within a short distance to the west. I have not yet, however, been able to find any exposure of the junction beds.

To the east of the Beacon ridge, towards Sutton, two exposures beside the deep cutting have been found. A good section of the soft sandstones occurs at Welshman's Hill on the sides of the road descending from Beggar's Bush to Banner's Lodge, and about a mile and three quarters from the cutting described. The beds are here exposed for some little distance to a depth of about 6 or 7 feet, and show soft sandstones, absolutely free from pebbles. They are light red in tint, with darker patches, and mottlings and streaks of reddish and yellowish grey. The contrasts of colour are naturally not so strong as in the freshly exposed beds described above. The false bedding can easily be made out, though partially obscured by weathering; and on the Park side of the road, where the

surface of the sand has recently peeled, several lenticles, with their markings, are very distinct. The dip here appears to be 2° or 3° *to the west*, and the sandstones lie distinctly below the Pebble Beds forming Welshman's Hill. Both this section and the cutting are well within the area mapped as Conglomerate by the Survey.

Another much slighter exposure of the sandstone occurs in a lane immediately to the S. of King's Standing. The sand is of varying tints of light red, is false bedded, and has some sandy wash containing pebbles above.

To sum up this section of my work, the following points seem to me conclusive that the beds described are the Lower Red and Mottled Sandstone, which forms the lowest member of the Bunter.

1.—The general character, softness, entire absence of pebbles, and the very bright and varying colours of the beds, the whole agreeing in every particular with Professor Hull's description of the Lower Mottled Sandstone in other districts.

2.—They are very markedly false bedded, generally coarse in texture, and, as in other districts, are characterised by the long lenticular or oblique bedding due to currents.

3.—They certainly lie beneath the true Conglomerate, and show an eroded surface.

4.—They are exposed at an horizon some 200 feet above the last bed mapped as Permian, and the chief sections lie clear away from the Permian area.

5.—They are completely dissimilar from the hard dull brownish purple and dirty brown grey sandstones, conglomerates, and clays, of the Permian of the district.

6.—A clear unconformity exists between these beds and the Permian Strata; the former having a dip to the east of not more than 3° or 4°, while the latter have usually a dip of about 15° to the S.E., and dips of 20°, 26°, and 40° are also given by the Survey directly W. of the Beacon ridge.

The Bunter Conglomerate, or Pebble Beds.—When we examine carefully the district to the east of the Beacon ridge,

and lying between it and Sutton, we find that all the higher grounds at least are composed of Pebble Beds; and the slopes and valleys, even where it is in a high degree probable that the Conglomerate does not exist, are also so strewn with pebbles, which have been gradually washed down from above, that it is easy to consider the whole district covered with Pebble Beds. This surface wash makes it extremely difficult, if not impossible, without boring, to map accurately the limits of the Pebble Bed division. Another difficulty arises from the fact, that, in many places, what is no doubt true drift, derived mostly from the Conglomerate, and in many respects like it, overlies the whole. But what is perhaps a more serious difficulty than either is due to the uneven surface upon which the Pebble Beds were deposited. The land was rising in the direction from which the waters came, and as the shore pushed its way out the currents were sufficiently strong to erode long channels in the loose soft sand previously deposited. Hence the pebbles were laid down over a worn and undulating surface, and any geometric aid to the mapping becomes of comparatively little use. Thus, in the district under consideration, it seems highly probable that the Pebble Beds sometimes lie in deep trenches and hollows, and that this, rather than any change of dip, or faulting of the beds, accounts for the fact that undoubted Pebble Beds often lie where we should expect the Lower Sandstone to exist. One such trench seems certainly to lie on the northern side of the cutting described above, a difference of level of some 30 feet marking the surface of the sandstone in about 100 yards. It may even be that in some parts east of the South Staffordshire Coalfield the Lower Mottled Sandstone has been removed entirely.

Again, as the land approached, we should expect floods and stronger currents to carry out small fragments of rock and some rounded pebbles from the Permian conglomerates to a greater distance; the result in the deposit being alternate layers of sand and angular and subangular fragments, and some pebbles. As the fragments would probably not have travelled

far, or have been rolled about on a beach, we should expect them to be more or less angular. Now this is exactly what we find in this district. As we approach nearer and nearer to the true Pebble Beds, small angular fragments begin to make their appearance, with only a few, mostly small, pebbles; but the conditions seem to have been so variable that in one place we have the gravels and shingle laid down abruptly upon the soft sand, while in another the process was clearly often a slower one, and we have the alternate bedding of sand and small fragments before mentioned.

Such a case is well illustrated in the large section exposed on the side of the Aldridge Road, immediately below the Beacon to the south. At the bottom we have fairly hard and compact thick-bedded sandstones of light red, banded with buff, yellow, and grey, showing a few layers here and there of coarse silt or small angular fragments. The deposition as a rule went on quietly and almost continuously; and in several places thin lines of red and purple clay separate the thick sandstone beds. Above, however, while the sands retain the same general character, they are inter-bedded with many lines and irregular bands of fragments but very slightly worn; these are sometimes horizontal, but more frequently cross the beds obliquely, and mark the current bedding. As we rise still higher, the beds of sand become thinner and thinner, and, as a rule, the lines of fragments are more numerous and more rounded, until, at about 20 feet above the bottom of the section, the compact coarse pebbly shingle comes on suddenly, with an irregular under-surface, and with a few larger pebbles embedded several inches in the sand bed below. In places, however, lines of rounded pebbles occur slightly lower, and have thin bands of sand above them, which are frequently contorted, indicating probably the soft and incoherent nature of the sand when the pebbles were laid down.

I believe, therefore, that we have in this section an example of passage beds from sandstone into conglomerate, laid down in

the quieter waters, while possibly the stronger currents flowed elsewhere and carried coarser material.

The angular fragments in the beds are often very peculiar, containing rocks which I have never met with in the rounded conglomerate; notably one of a very bright yellowish green, and extremely hard, like jasper, as well as numerous fragments of ashes and hornstone, apparently derived from the Permian Breccia. Bands of these angular fragments, some of several feet in thickness, seem to have been deposited in the lower beds of the Conglomerate on different horizons, and in all likelihood at particular places. Any way, there is no evidence to show that they are persistent over any large area. The layer of angular and subangular fragments lying below the coarse rounded Pebble Beds at Black Root Pool is probably one of these bands, and would appear to indicate that we are there not very far from the bottom of the Conglomerate. Derivation from the Permian Breccia would give it some of the characters of that formation. This conclusion other considerations go to strengthen.

The whole of the upper portion of the Beacon, then, as noted by the Survey, is made up of the beds belonging to the Conglomerate; but that the coarse shingle shown at the top of the section on the Aldridge Road is not continuous, seems clear from the fact that some distance along the ridge small exposures of light red sand may be found, in several places considerably above the level of the road.

From my work in the district, as far as it has gone, it is in a high degree probable that the Conglomerate beds descend eastwards from the Beacon ridge at about the same general dip as the underlying Lower Mottled Sandstone, and *form a gentle syncline beneath Sutton Park*, rising again to the east and south east. That the Conglomerate towards the eastern margin of the Park dips gently *to the west* instead of the east, may be seen in the railway cutting near the second bridge past Sutton Park Station; and the westerly dip of the beds at Welshman's Hill has already been referred to.

With regard to the thickness of the Pebble Beds, they can scarcely anywhere be much more than 150 feet thick, though further work, including the careful determination of some heights, is necessary to settle the point with exactness. To the east they are clearly much less. In some of the deeper valleys they must be absent altogether, and in some other places they can be little more than a shell. At Black Root Pool I do not anticipate that they are more than 60 or 70 feet. Taking Mr. Howell's estimate of 350 feet (the double estimate I do not understand) to indicate the thickness of all beds from the Permian to the top of Barr Beacon, I should think he is not far wrong; the lower sandstone being at its *maximum* thickness probably about 200 feet.

I hope to give in a future paper the results of further investigations, and to note many points of interest which cannot be given here respecting the surrounding district, as well as to compare the rocks with typical developments of the same beds elsewhere. One point I may mention. It seems not at all unlikely that much, if not all, of the strip marked as Upper Bunter Sandstone to the S.E. of the district we have been considering, will prove on careful research to be *Lower* Red and Mottled Sandstone, brought up by the rising of the synclinal curve.

It remains only to acknowledge the kind and patient help in the field which I have in many cases received from my friend, Mr. J. Udall, F.G.S., for which my thanks are due.

IX.—*Note on a New Drift Section at Northfield.*

By Joseph Landon, F.G.S.

Senior Lecturer in Saltley Training College.

[Read before the Geological Section, June 5th, 1890.]

In the construction of a new road from close to Northfield Station into the King's Norton Road, a cutting has been carried through glacial drift, in some places to a considerable depth, for the purpose of sewering and of obtaining road metal. The section exposed at the time I examined it, in company with Mr. Udall and Mr. Sherwood, was about 16ft. deep, and showed four distinct beds, as follows :—

1.—A band, about 18in. thick, of deep purple, homogeneous clay, occasionally streaked with greyish blue, and containing a few scattered and rounded pebbles, mostly small. This is clearly the same clay as that worked for brick making in the pit near the Bromsgrove Road, just beyond the Bell Inn.

2.—A bed, about 3½ft. thick, formed of a heterogeneous mixture of sands, clays, angular and sub-angular fragments, and rounded pebbles. The whole showed little or no trace of structure, and was dull in colour ; the prevailing tints being dull

red, ochre, and dirty purple. The pebbles were mostly quartzites of various colours, some of white quartz, and several of chert, with many partially rounded pieces of sandstone, &c. The most characteristic rocks represented seemed to be fine white and yellow sandstones, almost certainly Carboniferous; blue-banded hard shale, usually rounded; and hard purple Permian sandstone. The angular fragments were mostly small and extremely varied in kind. The last six inches of the bed contained a considerable amount of clay in patches, and this was followed immediately by an irregular line of black sand (not coal, though fragments of this occur) varying in width from $\frac{1}{4}$ to $\frac{1}{2}$ inch.

3.—A four-foot layer made up, in many respects, of similar materials to the bed above it, but showing a distinctly bedded structure, as though laid down by water. Pieces of hard green and blue mudstone and shale, and white quartz pebbles were conspicuous, with numerous pieces of deep purple sandstone—some probably Permian, others certainly not so. Pieces of greenish and purple volcanic ash were not uncommon, and similar to specimens found in the Permian Breccia. Neither of the bands described contained more than occasional examples of the larger erratics. Small angular fragments constituted by far the larger part of the finer material.

4.—A roughly-bedded mass, extending to a depth of about eight feet, composed of finer gravels, patches of red and purple clay, and a little sand, with much fine angular rubbish. Scattered throughout the whole were many larger erratic masses, which became more numerous towards the bottom. The rock masses included a very varied assortment of sandstones—Permian being especially prevalent near the top—limestones, shales, mudstones, hornstones, felsites, ashes, chert, &c. Nearly the whole of the erratics presently to be described came from this layer. The bottom of the stratum was, of course, not reached in the working, but about twelve months ago a well-sinking, further along the same ridge, showed exactly similar characters, and was there continued to a depth of about fifty

feet when the New Red Marl was reached. As in the section described above, erratics were very numerous.

During the progress of the road-making large heaps of pebbles and erratic blocks were taken out to be broken up for road metal. Usually they were from a few inches to a foot or more across, but many larger masses lay scattered about, a dozen or more having a length of two feet. Still larger ones occurred, and one very large one, but these the workmen were unable to remove. On the sides of the road at the time of our visit were numerous rounded pieces of hard blue shale or mudstone, and many of these showed parallel flutings, grooves, and scratches of the most perfect description. In no single case did I observe one of the harder rocks either grooved or scratched.

Days might easily have been spent in the examination of the large heaps of erratics and pebbles, but a few hours' work showed the following rocks, in many instances repeated again and again :—

1.—Quartzite pebbles of the usual types and colours, blue and white quartz pebbles, sub-angular quartzose masses, and several large pebbles which were once conglomerate, but now filled up into solid silica.

2.—Permian sandstones of at least five distinct varieties, including the lavender basement beds, the calcareous grits from the middle and the top of the series as shown at Clent, brown sandstones like those in Doctor's Coppice cutting, and bright red sandstones with large white patches which may be found *in situ* in the same section.

3.—Bright purple sandstones of apparently three kinds, certainly not Permian, but possibly Cambrian.

4.—White and yellow sandstones, showing considerable variety of texture. Some were clearly millstone grit, others probably were from the Upper Coal Measures. One fine sandstone contained plant remains identical with those in the rock very slightly exposed in the stream near Balam's Wood at Rubery.

5.—May Hill sandstone with the usual shell casts.

6.—Green sandstones of several kinds, some pretty certainly from Halesowen. One, dark green, hard and compact in texture, was very probably Holly Bush. A light green coarser sandstone was quite unknown to me.

7.—Volcanic ashes of several kinds. These were mostly similar to those in the Permian Breccia, a dull green and a purple variety being not uncommon.

8.—Conglomerates of various texture, from a large hard block, with pebbles over an inch in size, to the finer kind with white quartz pebbles associated with the millstone grit. One looked much like a bed found sometimes at the base of the Llandovery; another almost identical with the Permian Conglomerate of Barnford Hill.

9.—Hornstones and cherts, the latter sometimes showing on the outside the peculiar rough or porous appearance which is not uncommon. Some of the hornstones certainly looked like those of Lilleshall Hill.

10.—Basalts. Some were quite fresh, some rounded and weathered a rusty yellow to the depth of an inch.

11.—Felsites. These were very common, the ordinary Welsh blue variety being occasionally so weathered that nothing but a kernel of the original rock remained, while other blocks seemed but little altered. A hard and much darker quartz felsite, with crystals of quartz, sometimes $\frac{3}{16}$ in. across, was also met with.

12.—Mudstones and shales, mostly lightish blue, a few green, and one black. These were often grooved and striated. Some dark dull blue fragments were probably slates.

13.—Criffel granite. Two specimens of this rock were met with, one about 6 inches in diameter, the other larger. Both were very much rounded.

14.—Limestones of a number of different kinds occurred. The crystalline varieties were light grey, brownish grey, ochre, and one dark blue grey. Those with dull clayey look were deep blue or nearly black. Some were clearly Silurian, others

Carboniferous. A single purple specimen was like the limestone near Lilleshall Hill; and another, dark blue in colour, might have been mistaken for a basalt but for the fossils. The ochre coloured ones may be Hoar Edge.

15. — Andesites. Several blocks found were almost certainly true andesites. A dark hard variety, breaking with a sharp conchoidal fracture, was very like one occurring in the Lake District. Another dull green one of similar character was almost exactly like the andesite of Llanfawr, near the Corndon.

Besides the above, several unknown rocks were met with, notably a brownish black coarse sandstone with red spots; and a green glassy-looking rock, possibly a limestone.

X.—*Bimetallism: How its Benefits (and other Public Advantages) may be Attained and its Evils Avoided.*

By Alfred Hill.

[Read before the Society, May 7th, 1890.]

[An article by Professor Alfred Marshall appeared in the "Contemporary Review," of March, 1887, proposing a note currency based on gold and silver jointly; and in the same year Mr. A. Beckett Terrell suggested in a pamphlet a somewhat similar plan. Both these schemes, however, differ greatly from the one now proposed.]

The increased demand for gold caused by demonetization of silver in Germany, &c., and the return of the United States to cash (*i.e.*, gold) payments, seem to be the main cause of the rise in the value of gold relatively to silver; and the fact that, in India, where the currency is silver, there has been no general rise in price, seems to show that the change is owing to the appreciation of gold rather than to the depreciation of silver. It is highly probable that this appreciation of gold had much to do with causing the depression of trade, which was general, until recently, in commercial countries—though the great fall in the prices of some articles, such as copper, wheat, &c., must have been partly occasioned by other causes. It also has increased the burden of the National Debt. For a continual fall of prices acts against the man who buys to sell again, in which position are manufacturers as regards the raw material. It is, therefore, highly desirable to check this progressive fall of prices. The restoration of silver to its old place in the world's currency would, probably, have that effect; but such a movement could not be made without the general concurrence of nations, which is improbable; and indeed such arrangements might

produce serious political entanglements. A plan, therefore, which this country might adopt without foreign concert seems to be the desideratum.

Long before bimetallism became a question, the late Mr. Edwin Hill, of Somerset House, being greatly impressed by the ruin caused by the English monetary panic of 1847, put forth a scheme of currency which he conceived would, if adopted, prevent the recurrence of that disaster. Seeing that the panic arose from the dread of an insufficiency of the circulating medium, *i.e.*, of legal-tender money (gold and Bank of England notes), Mr. Hill proposed that Government should issue legal-tender notes, inconvertible, but which should never be depreciated—two qualities which not only can be, but actually *have* been, combined. For during the four first years of the Bank Restriction Act of 1797 (which forbade the Bank of England from paying its notes in cash, while placing no restriction on its power of issuing, thus putting an end to convertibility) the notes remained at par; and had the Bank continued to keep its eye on the bullion market, and, when that rose, checked its issues, this happy state of things would have continued. The value of silver coins in the countries of the Latin Union (Belgium, France, Switzerland, Italy, and Greece) is kept up to a token-value (twenty francs to a golden Napoleon) *by restricting the coinage*, while the coins of the other franc-money countries have to be sold at the money-changers in the Latin Union at from 20 to 30 % discount. Seeing, therefore, that the value of such notes must depend on the relation of supply and demand—so that if the amount in circulation were kept in careful regulation, neither depreciation nor appreciation could occur—Mr. Hill proposed that the notes should be issued in payment for stock in the funds, which Government should, from time to time, purchase in the market, and which stock should be transferred into the names of the Commissioners for the Reduction of the National Debt, and placed by them to a special account. Such notes would rapidly come into circulation, for the sellers of the stock must, of course, pass them off; and, indeed, the

experience of Scotland and Ireland proves that, where the public has the option, it *prefers* one pound notes to gold, even when the former are not legal tender; and thus, as the notes came into circulation, sovereigns would be melted down until all were gone. While both notes and sovereigns were in circulation, they would obviously pass indifferently, as Bank of England notes and gold do now; but when all the sovereigns had disappeared, it would be necessary to regulate the value of these inconvertible notes. So Mr. Hill proposed that every week affidavits of the state of the market, made by persons eminent in the bullion trade, should be publicly brought before some high judge—as the Lord Chancellor—and if it appeared that gold had a tendency to fetch more than the mint price (£3 17s. 10½d. per ounce of standard gold) the judge should order a *sale* of stock to be made by the Commissioners. Thus, the notes received in payment for such stock being withdrawn from circulation, equilibrium would be restored. If the notes showed any tendency to appreciation a *purchase* of stock by the Commissioners would be ordered, which would be paid for in notes drawn from the Government store, thus increasing the amount in circulation. Practice would soon make this regulation easy. There is no danger of mis-statement of the market price, or wrongful order of the judge, as anything of the kind would be immediately detected. And thus the desideratum of a currency, inconvertible, but not subject either to depreciation or appreciation, but always coinciding with the price of gold, would be attained, and the commercial world would be relieved from all the evils and anxieties caused by variations in the stock of gold. As the supply of currency would be thus always kept equal to the needs of trade, the money market would be no more affected by the exportation or importation of gold (or by the amount in the Bank coffers) than by that of any other commodity; and thus one source of complexity at all times, and of great mischief and danger at some, would be dried up.

But now to *Bimetallism*—

Mr. Hill's scheme was broached long before the question of bimetallism arose, and without any view to it; but, by a simple alteration, this plan may be made the means of establishing a double standard. Instead of looking only to the price of gold, that of *silver* might also be taken into account—a weight of silver, equal in market value (at the time of the adoption of the scheme) to an ounce of standard gold, might be fixed, and when the price of an ounce of standard gold, *plus* the prescribed weight of silver, amounted to more or less than double £3 17s. 10½d. (£7 15s. 9d.), stock should be bought or sold as proposed above. Thus, a bimetallic standard would be adopted free from any possible evil arising out of premium on gold or silver. But as the proposed notes would be based half on the value of silver, a rise or fall in that metal would affect the amount of gold they would purchase, so that while sovereigns remained in circulation, those would not be exactly equal in value to the one pound notes. A *fall* in silver would place sovereigns at a premium, and thus cause them to disappear from circulation, their places being taken by notes, while a *rise* in silver would bring them to a *discount*. The latter evil, however, might be prevented by the mints refusing to coin gold, in which case sovereigns would pass as tokens only, as do the silver coins in the countries forming the Latin Union. Although this scheme, if adopted, would create no demand for silver, it would demonetize a vast quantity of gold, calculated at, from £60,000,000 to £124,000,000. So large an amount of gold being thrown upon the market within a few years must cause a depreciation in the value or purchasing power of that metal, which must lower its price, and consequently raise the gold-price of silver. And on the working of the system proving satisfactory, its advantages would probably induce other nations to follow our example, and thus gold would be demonetized to an immense extent, causing its important depreciation and a consequent rise in the price of commodities, including silver.

The two metals combined would certainly afford a more stable basis of currency than either of them alone, since both are not affected at the same time by the same causes: and thus their respective fluctuations would tend to balance each other, and a steadier scale of prices be maintained.

Such a change in currency must tend greatly to check the tendency of prices to fall, and thus encourage enterprise and increase employment; and vast benefit would occur to this nation by the enormous amount of stock dividends of which it would become possessed—from £1,800,000 to £4,000,000 yearly. Indeed, if this sum were placed annually to a sinking fund, to accumulate at compound interest (at 3 per cent. per annum), it would pay off the National Debt in from about 67 to 90 years! The foregoing scheme would, of course, require to be established by legislative authority, including some arrangement with the Bank of England, which, if it continued to issue notes, must be empowered to pay them in Government notes, and to hold those, instead of gold, against its issues.

Discussion on Mr. Hill's Paper.

Mr. Barham:—No scheme of currency can be sound which is not founded directly on the precious metals. Mr. Hill's scheme is founded on the opinion of certain official people as to whether the country is sufficiently supplied with notes. It makes no provision for a supply of the precious metals for the satisfaction of foreign nations. The value of notes must be tested by direct reference to the Standard. They must always be convertible on demand into the Standard.

Theoretically the Standard might consist of a series of articles taken at their market prices, and a Standard of this sort might be applicable to rents and other charges fixed for many years, and would perhaps be more constant in value than the precious metals. But for practical purposes for testing the value of notes, the Standard can only consist of the precious metals. Notes to be soundly constituted must be exchangeable on demand into the precious metals, so that

those who have payments to make abroad may be sure to be able to obtain what gold or silver they require. The adjustment of balances in this way between foreign nations by remittances of bullion is continually going on, and the value of every article is thus referred to the precious metals. If both these metals are to be used by a country in combination, as the bimetallists desire, it appears to me that it would be necessary to maintain a sufficient supply of both metals to meet any demand. If the cost of production of either metal should fall below that at which the ratio between them was calculated, then cheaper metal would be brought to the mint and the dearer metal taken away, a process that could only end by the country becoming monometallic with the cheaper metal as the Standard. We shall see whether the proposed legislation in the United States has this result.

But if the richer and more powerful nations were to combine and receive the cheaper metal, giving out the dearer, the dearer metal would come upon the open market in quantities and would fall in value, so that the ratio would tend to be readjusted. and I do not doubt that moderate fluctuations in the supply of the metals might be adjusted by agreement in this way, and the bimetallic method might work well enough in normal times. It would have the advantage that the Standard would tend rather to fall in value; a rising Standard such as we have suffered under lately has very injurious effects upon industry, continually increasing the weight of debts, improving the position of mere capitalists, such as the holders of debentures, at the expense of the active and industrious workers, who stand in the position of ordinary shareholders. Reduction in the price of articles of consumption, which is brought about by improved methods of supply or good harvests, is of course most beneficial; but when prices are reduced by a rising Standard, the reduction is brought about by a transfer merely from one class of people to another. In the case of a rising Standard, it is the working people who suffer, and the idle classes, who invest in securities with fixed interest, who

benefit at their expense. This would be, I think, the great advantage of bimetallism, that it would maintain a Standard very constant in value, though tending rather to fall as the metals become more plentiful, the cheaper metal doing the work. But if either metal became greatly increased in quantity, as was the case when the Californian and Australian gold fields were discovered, there would be a great hoarding of the dearer metal, and the ratio could not be maintained; also, if war threatened, gold would be hoarded by the military powers, and I fancy Napoleon or Bismarck would care little for bimetallic agreements if the filling of military chests was in question. Altogether I believe that the more sound method is for some nations to have a gold and some a silver Standard, and for nations like ourselves, with great business on all sides, to arrange, as far as possible, to receive and pay over both metals freely, but to commit ourselves to no agreement except that we will pay what we owe in gold to all who ask us to do so.

Major-General Phelps:—The first impression that is left by this paper is, that it is written on a state of things that has long since passed away. Modern conditions are quite changed from those of the last century, and the idea of the Lord Chancellor plunging with his affidavit-swearing bullion-brokers into the maelstrom of city finance—

rari nantes in gurgite vasto—

seems scarcely practical. Such a scheme might possibly work if England were to cut herself off from all foreign countries, and to devote herself to the unprofitable course of keeping an inconvertible paper currency going by a system of rigid Japanese seclusion.

But England is a shop-keeping country, and it is her first interest to have the access of her customers to her markets at all times free. Things are not made easy for such of her customers as come from silver-using countries, when she refuses to take the money they happen to have in their pockets in payment for her goods. When she sends them away to

change that money for gold, she takes the perilous step of suggesting the consideration whether the more obliging shopkeeper over the way, who is not above taking silver money, is not more conveniently worth patronizing.

The difficulty of Mr. Hill's proposal thus comes in when the foreign exchanges are considered, the balances of which are a sort of fringe to the exchange of commodities between us and other countries, and may be said to include the profit or loss on the transactions. The proportion of bullion to paper in settling these transactions, internal and external, is infinitesimally small, the transactions of London Bankers' Clearing House (where the weekly clearances far exceed the full-value metal money of England in value) far exceeding the amount of the National Debt. But small as is the proportion of bullion to paper in the amount of money used to adjust these exchanges, it is indispensable that the one should at all times be freely convertible into the other. The scheme before us, if I understand it rightly, would drive most of our bullion out of the country; and this would make settlements with our foreign customers more difficult than they now are.

The effect of placing exchange obstacles between us and our customers in silver-using countries is plainly illustrated by the results of demonetizing silver on the Manchester trade in coarse cotton yarns with India. The development of a lucrative trade has been arrested, scores of mills have been built in India since 1872 to spin this kind of yarn, and weave cloth with it, and their manufactures not only displace our goods in India, but upon the east coast of Africa and in China and Japan. The adoption of the scheme before us would, I think, intensify this difficulty, and still further handicap our unfortunate manufacturers.

It seems to me that the State may either refuse to have anything to do with coining metallic money, and leave this function to individual merchants, subject to the ordinary laws as to adulteration and weight, or it may coin the metal or metals that it finds in actual use among its customers. In the

latter case it seems illogical for the State to select one metal for exclusive dealing. If it undertake the duty of coining metal money, it should practise Free Trade at its Mint, and ascertain and announce the rates, in terms of each metal, at which it will receive, stamp, and issue its coins. Thus only can it avoid giving an inexpedient preference to some of its customers and hampering others.

The late rise in the gold price of silver, on the rumour of free coinage of the metal in the United States, shows that the only effective demand for bullion used as money is that of Governments. No other demand acted upon the price of the metal, but the prospect of a demand for coinage as full-value money and legal tender sent up the gold price of silver several pence per ounce. And the experience of the Latin Union from 1816 to 1872 proves that the convenience of a fixed ratio can easily be secured by agreement between even a limited number of Governments.

With reference to periodical panics, which are referred to in Mr. Hill's paper as having ceased, it may be argued that panic has become chronic, as may be seen in the arrested development of trade since silver was disestablished in Europe as full-value money.

Instead, therefore, of adopting the highly artificial system of interference with the coinage of money, as proposed in this paper, it would be better for us to introduce the principles of Free Trade into our Mint, and to coin whatever bullion our customers may use as full-value money. We may be pretty certain that those principles will serve us as well in the Mint as they have done in every other department of our commercial life.

Mr. G. H. Sargant :—I do not propose to discuss the general subject of bimetallism, which appears to me too wide for the present occasion, but to hark back to the particular

scheme unfolded in the paper to which we have listened to-night.

Whether we approve or disapprove of that scheme, one thing seems clear—that such a scheme has no hope of ever being accepted. In England we do not introduce such a change, except under pressure of absolute necessity. Now the main object of Mr. Hill's scheme is to prevent panics; but panics have already ceased for a quarter of a century, and there are several reasons why they should not recur.

Panics arise partly from scarcity of the circulating medium; one reason therefore for their cessation is found in the great extension of the circulating medium by the growth of the use of cheques.

Panics also arise from a sudden loss of trust in the stability of banks. During the period when panics recurred periodically, banking was carried on by private persons; but since the passing of the Companies Act, 1862, banking has fallen more and more into the hands of joint stock companies; and one of the most valuable provisions of that Act requires every joint stock bank to make public a properly prepared statement of assets and liabilities every six months; so that customers need no longer rely on their faith in the integrity and financial soundness of the private owners of the bank, but, if anything should occur to shake their faith, may refer to the published accounts. The value of these published accounts does not end here; the necessity of exhibiting at short intervals the condition of their assets precludes managers from locking up their funds and so endangering a stoppage of payment.

Another reason for the cessation of panics arises from the broadening of the basis of speculation. The two causes I have already mentioned, want of sufficient circulating medium, and loss of faith in the stability of banks, arise together in times of over-speculation. When speculation was confined to two or three kinds of stock in one market, any panic affected all speculators, and, through them, nearly all banks; but now,

when there are innumerable classes of stocks, and the markets of several nations are practically joined by the telegraph, a panic may occur in one market, or in one or two classes of stocks, without affecting the rest. Thus of late we have seen panics in the copper market, in South African stocks, in nitrate shares, on the Berlin Bourse after the resignation of Bismarck, and many others within a short period, but none of these produced any serious effect outside their own circles.

It is therefore evident that as a remedy for panics Mr. Hill's scheme is unnecessary.

The main proposal of the paper is in fact a proposal for a forced currency, which is impossible in a democratic country. The proposal is to regulate the exchange value of a new currency (to consist of inconvertible notes), by buying or selling Government stock; but it would be impossible to force the holders, or purchasers, of stock to accept, or give, these notes. A stock holder, who wished to convert his investments and divert his capital to foreign trade, would refuse to take anything but gold; if the notes became at all unpopular, stock holders and purchasers would refuse to deal except on a gold basis, and the whole scheme would fall to the ground. Indeed, the same result would probably soon be brought about by speculators who would "bull" and "bear" the notes alternately. And if means were found to circumvent such movements, and the notes were actually put into circulation, yet, if at any time they became unpopular, they would be handed back to the Government in taxes.

The whole scheme therefore appears to be impracticable, as well as unnecessary.

As to the bimetallic proposal, Mr. Hill's scheme is one for a wholly undesirable national bimetallism (for no foreign nation would accept our inconvertible notes), and, as such, appears to concentrate the disadvantages, whilst excluding the advantages of, true bimetallism. Two metals are in actual use as money amongst civilised peoples; and, it being obviously

undesirable, if not impossible, to abolish either, a double Standard is proposed so as to establish an international currency for international trade.

In supporting the vote of thanks to Mr. Hill for the very interesting paper he has laid before us, I consider that our gratitude is due to him, not only for the paper, but still more, for the valuable discussion which he has raised.

XI.—*The Structure and Function of the Air-bladder in certain Fishes.*

By Professor T. W. Bridge, M.A.

[Read before the Society, June 4th, 1890.]

In the great majority of Teleostean Fishes there is situated on the dorsal side of the cœlome or body cavity, between the alimentary canal below and the kidney and vertebral column above, a more or less elongated sac, with membranous walls and gaseous contents—the air-bladder. Primarily developed in the embryo as a cæcal outgrowth from the dorsal side of the alimentary canal in the region of the future œsophagus, the air-bladder grows to some extent forwards, but to a greater extent backwards, and may either permanently retain its primitive connection with the alimentary canal by means of a longer or shorter contracted tubular canal—the ductus pneumaticus—or become entirely separated therefrom in the adult by the complete atrophy of the duct, or its degeneration to the condition of a solid fibrous cord. The presence or absence of a ductus pneumaticus in the adult, in correlation with other distinctive features, has led to the division of Teleostean Fishes into two main groups—the Physostomi, which includes such well-known forms as the Salmon, Eel, Carp, and Herring; and the Physoclisti, of which the Cod and the Perch may be taken as equally familiar examples. The former group is characterized by the retention of the primitive embryonic communication between

the air-bladder and the œsophagus throughout life, and the latter by the absence of any such communication in the adult form—the result of the partial or complete atrophy or solidification of the ductus pneumaticus. But this distinction is very far from being an invariable one, inasmuch as well-marked exceptions occur in both groups. On the authority of Kner (1), it may be stated that in the Physoclist genera *Holocentrum*, *Priacanthus*, *Cœsio*, &c., a permanently open ductus pneumaticus is present, and the same remark apparently applies also to some Gadidæ (*e.g.*, Lota vulgaris). Similarly, it may be pointed out, on the authority of Bridge and Haddon (*l.c.*), that there are certain Physostomi (*e.g.*, some Siluridæ) in which the ductus pneumaticus is either absent altogether or is represented only by a thin, solid, fibrous cord. Moreover, it must be remembered that in the embryos of all Teleostean Fishes, whether eventually Physoclisti or Physostomi, a ductus pneumaticus is present—a fact which necessarily follows from the mode of development of the air-bladder.

It is interesting to note that, in at least two instances, the air-bladder has acquired secondary means of communication with the exterior other than by means of the ductus pneumaticus, the œsophagus and the mouth; and of these one example occurs in the Physoclist and the other in the Physostome divisions of the Teleostei. Moreau (2) has described in the "Horse-mackerel" (*Caranx trachurus*) a peculiar duct, or "canal de sûreté," which leaves the dorsal side of the air-bladder, and opens by an extremely small orifice into the right branchial chamber. In the Herring (*Clupea harengus*) a short tubular canal leaves the hinder end of the air-bladder, and opens externally on the left side of the genital aperture behind the anus. In *Caranx* the true ductus pneumaticus is absent altogether, but in the Herring the posterior canal co-exists with a normal ductus pneumaticus leading from the air-bladder and opening into the posterior extremity of the stomach. In neither instance is anything known as to the mode of formation or morphological nature of these secondarily acquired ducts.

Structurally, the walls of the air-bladder are formed of two well-defined layers—the tunica externa and the tunica interna. The tunica externa is composed of fibrous tissue, with an admixture of elastic fibres, and, moreover, can generally be resolved into two more or less definite strata, distinguishable from each other by differences in the course and arrangement of their component fibres. The tunica interna, on the other hand, consists of a basis of connective tissue, with a coherent epithelium of flattened cells on its inner surface In addition to its proper wall, the air-bladder is always invested on its ventral surface, and sometimes also on its lateral surfaces, by the peritoneum lining the interior of the abdominal cavity.

The principal modifications observable in the structure of the air-bladder in different Teleostei relate to variations in relative size and shape, to the appearance of external transverse constrictions whereby the organ becomes divided into two or more inter-communicating chambers, to the formation of hollow, laterally-situated, cæcal branches, and also to the extent in which its cavity is completely or incompletely sub-divided by the development of secondarily formed, internal, longitudinally or transversely arranged, fibrous septa. In not a few Physoclist genera the air-bladder is completely suppressed (*e.g.*, the Flat Fishes, or Pleuronectidæ). In proof of the extreme variability of the air-bladder, even in different species of the same genus, it may be mentioned that in some species of the Physoclist genus *Scomber* the air-bladder is absent altogether while present in others. The genus *Polynemus* also affords examples of a similar kind. In some *Siluridæ* among the Physostomi the air-bladder may be well-developed in certain species, but be represented by a mere rudiment in others of the same genus. In fact, many of the variations in the structure of the air-bladder, to which attention has just been directed, may occur in different genera of the same family, or even, not infrequently, in different species of the same genus; and it may also be added that well-marked individual variations in the relative size of the organ are far from being at all uncommon.

A very characteristic feature in the structure of the air-bladder in many Teleostei belonging both to the Physoclist and Physostomous divisions, but more especially in the former group, is the existence of peculiar aggregations of capillary blood-vessels, or "retia mirabilia," which are either simple radiating tufts imbedded in the wall of the air-bladder, between the inner and outer tunics and immediately external to the epithelial layer, or form conspicuous flesh-coloured projections into the cavity of the air-bladder, which are invested by a special modification of the normal flattened epithelium lining the interior of the organ. Such retia mirabilia are found in both divisions of the Teleostei, as, for example, in the Perch and Cod among the Physoclisti, and in the Eel among the Physostomi; but it must be remarked that not infrequently such structures are entirely absent in certain families (*e.g.*, the *Siluridæ*) belonging to the latter group.

In certain families of Teleostei the air-bladder exhibits a marked tendency to give off lateral branches, which may again divide and sub-divide, but ultimately terminate in blind cæcal extremities. Such singular modifications are very general in the Physoclist families of the *Sciænidæ* and *Polynemidæ*. The lateral cæcal branches of the air-bladder exhibit the greatest possible variety both in number and arrangement, sometimes being very numerous and uniting with those of the opposite side to found a series of dorsally and ventrally arranged tubular arches on the dorsal and ventral surfaces of the air-bladder respectively (*e.g.*, in the Sciænoid *Collichthys*); or, as in the genus *Otolithus* of the same family, sometimes restricted to a pair of simple blind cæca at each of the antero-lateral angles of the air-bladder; or even, as in some *Gadidæ*, to a single pair of antero-lateral unbranched cæca, which may be directed forwards towards the skull.

Perhaps the most interesting modification which a branching air-bladder may undergo, is the tendency of certain of its cæcal branches to enter into intimate relations with the internal ear. In the genera *Holocentrum* and *Sargus*, belonging

to the Physoclist families of *Berycidæ* and *Sparidæ* respectively, two cæcal processes are given off from the anterior end of the air-bladder, which diverge as they pass forwards, and ultimately become applied to the fibrous membranes closing what would otherwise be vacuities in the outer walls of the auditory capsules. Parker (3) has also described a substantially similar arrangement as existing in the Red Cod (*Lotella bacchus*) amongst the Gadidæ, whereby two horn-like cæcal outgrowths from the anterior extremity of the air-bladder become intimately applied, not only to the membranes closing the auditory fontanelles, as in the two preceding genera, but also to the external skin. Many years ago, Weber (4) described a similar but somewhat more complicated arrangement which exists in the Herring. In this fish the anterior end of the air-bladder is produced into a slender canal, which, at the base of the skull, divides into a right and left branch; each branch traverses a canal excavated in the side walls of the hinder part of the skull, and again subdivides into a short posterior and a longer anterior branch. From Weber's figures (*l.c.*, plate VIII., fig. 64), it would appear that the two secondary branches expand terminally into two small globular enlargements, each of which is enclosed within a bony vesicle or chamber of corresponding shape, excavated in the substance of the opisthotic and prootic bones of the periotic capsule. Into the anterior bony vesicle a cæcal outgrowth from the saccular portion of the internal ear also enters, and becomes closely applied to the corresponding globose sac in which the anterior of the two branches from the diverticulum of the air-bladder terminates. With these modifications of the air-bladder and internal ear, by which the two structures are brought into such close anatomical and physiological relations, there is correlated a peculiar connection between the two sacculi of the internal ear by means of a transverse canal, which passes between these structures, crossing the floor of the cranial cavity beneath the brain. It will be noticed that the existence of a more or less intimate connection between the air-bladder and the internal ear by the simple

apposition of mutual outgrowths occurs in both divisions of the Teleostei, but also that the connection is of a more complicated character in the Physostomous Herring than in any of the Physoclisti, in which, so far, it is known to exist.

In certain families of Physostomi, the connection between the air-bladder and the internal ear becomes much more intimate than in any of the preceding examples, and is brought about by the intervention of a much more highly specialized and complicated mechanism, consisting of a double chain of three moveably inter-connected ossicles—the so-called "auditory ossicles" of Weber (4)—of which the most posterior on each side is imbedded in the corresponding half of the dorsal wall of the air-bladder, while the most anterior is in close relation with certain special outgrowths from the membranous labyrinth of the internal ear. With the existence of this singular connection between structures, apparently as remote physiologically as they undoubtedly are in their mode of evolution, there are correlated certain peculiar modifications both in the structure and the histology of the internal ear. The families in which the so-called "auditory," or, as it is perhaps better to call them, Weberian ossicles, are present, are the *Cyprinidæ*, the *Siluridæ*, the *Characinidæ*, the *Gymnarchidæ*, and the *Gymnotidæ*. With the exception of a very few species, all are of fresh water habitat, and with the exception of the *Cyprinidæ*, which are, for the most part, characteristic of the temperate or north temperate regions of North America, Europe, and Asia, are confined to the tropical and sub-tropical regions of both hemispheres. As the presence of so highly specialized and so unique a mechanism as the Weberian apparatus in this assemblage of Fishes is eminently suggestive of their community of descent, Sagemehl (5) has proposed for them the collective name of Ostariophyseæ (*οστάριον*, ossicle; *φύση*, air-bladder). In all the Fishes included within this group the number and arrangement of the Weberian ossicles, as well as their relations and mode of connection with the air-bladder and the internal ear, are substantially similar; although comparatively unimportant variations

may occur in the different families, due sometimes to the varying degrees in which the mechanism is specialized, and sometimes to the results of degeneration, following upon the disuse and consequent atrophy of certain parts of the mechanism.

The gaseous contents of the air-bladder consist of oxygen and nitrogen, but the relative proportions of the two gases may differ in different Fishes, and even in the same Fish at different times and under different conditions. Under normal conditions the proportion of oxygen is considerably less in fresh-water Fishes than in marine, and of the latter the amount of oxygen is enormously greater, amounting in some cases to 87 per cent., in deep-sea species, as compared with their shallow water congeners. A mere trace of carbon dioxide may sometimes be detected. The gases present in the air-bladder are derived from the blood as it circulates through the capillaries in the walls of that organ, and probably by some process of secretion in which the epithelium of the inner surface of the bladder plays an active part. The secretion of oxygen into the air-bladder has been investigated by Moreau (2), and from his researches it would appear that increase in the amount of oxygen present in the air-bladder as the result of augmented secretion may result from (*a*) the repeated artificial abstraction of gas from the bladder by puncture, or, where a ductus pneumaticus is present, by placing the Fish in water under an air-pump; (*b*) an increase in the external pressure to which the Fish is subjected, for, as Biot (6) states, the proportion of oxygen augments in proportion to the depth at which the Fish is found; or (*c*) by section of the sympathetic nerve. Gases may not only be secreted from the blood into the air-bladder, but may also be reabsorbed from the air-bladder into the blood. The capacity of the air-bladder for reabsorbing oxygen often renders that organ an accessory to the gills in the function of respiration, inasmuch as many Fishes, when placed under conditions involving partial asphyxiation, that is in water containing but little oxygen, may utilise the oxygen stored up in the air-bladder for respiratory purposes. Absorption of gases from the air-

bladder may also depend upon conditions other than those connected with deficient respiration, for just as an increase of external pressure may lead to the increased secretion of oxygen into the air-bladder, so a diminution of such pressure, leading to a marked distension of the bladder, may be followed by a corresponding increase in the activity of the absorptive process, whereby the over-distension is eventually relieved. Nitrogen may be considered as the complement of the oxygen present in the air-bladder, their quantities reciprocally varying from zero to a high percentage, at all events under experiment; but in a state of nature nitrogen is only present in large proportion in those Fishes that live near the surface and never sink to any considerable depth, that is, in the condition when the Fish only possesses a quantity of air small relatively to the quantity which it possesses when taken from a greater depth.

The rapidity with which the secretion and absorption of gas into or from the air-bladder take place to a large extent depends upon the presence or absence of retia mirabilia in the walls of that organ, both secretion and absorption taking place much more rapidly in those Fishes in which retia mirabilia are present, and with comparative slowness in Fishes in which those structures are absent. Moreau (*l.c.*) states that a Fish, which in a normal state had a determined proportion of oxygen, if placed in water deprived of oxygen, may lose by absorption nearly all the oxygen previously contained in its air-bladder, and this capacity for gaseous absorption is much more marked in Fishes with retia mirabilia. For example, in Carps, which normally possess less than 10 per cent of oxygen, the final amount at the conclusion of the experiment is almost the same; but Perches, having normally about 20 per cent of the gas, if similarly treated, lose almost every trace of the oxygen previously present in the air-bladder; and this difference in relative capacity for gaseous absorption in the two Fishes seems to depend mainly on the absence of retia mirabilia in the former, and their presence in the latter. Conversely, the capacity for the secretion of gas varies to much the same extent

and for the same reason. Thus Moreau (*l.c.*) states that in a Tench (*Cyprinus tinca*), and a Caranx (*Caranx trachurus*), from which a large proportion of the gases contained in the air-bladder had been artificially removed, the former, having no retia mirabilia, took several days to restore the abstracted air, while the Caranx, in which retia mirabilia are present, required only a few hours. Hence, from such experiments as these, and from others which need not here be specially detailed, Moreau arrives at the conclusion that the retia mirabilia serve equally well for both the absorption and secretion of gas, and that most Fishes provided with air-bladders may, by the processes of secretion and absorption, augment or diminish the amount of gas therein contained, and also that the rapidity with which such processes take place varies according to the presence or absence of the retia mirabilia.

The physiology of the air-bladder is almost as varied as its morphology. The primary function of the organ is, undoubtedly, that of a hydrostatic organ or float, with the special object of rendering the Fish, bulk for bulk, of the same weight as the medium in which it lives, thereby giving to the body of the Fish the same specific gravity as the water. By the possession of an air-bladder the Fish acquires, within certain limits, a capacity for exceptional freedom of movement with a minimum of muscular effort. But although evolved primarily as a hydrostatic organ, there can be no doubt that not infrequently the air-bladder subserves other functions in addition. Thus Dufosse (7) and Moreau (*l.c.*) have shown that in certain Fishes the air-bladder functions as a sound-producing organ. According to Jobert (8), the air-bladder is sometimes subsidiary to the gills in the function of respiration, since it inhales air through the ductus pneumaticus from the mouth, as in certain Brazilian Fishes (*e.g.*, *Sudis gigas*, &c.). More frequently the air-bladder is an auxiliary to respiration by acting as a reservoir for the superfluous oxygen taken into the blood by the gills, such oxygen being rapidly reabsorbed from the bladder into the blood, and used up for respiratory purposes when the Fish is in water

containing but little of that gas in solution. Other and more problematical functions ascribed to the air-bladder by different writers will be referred to subsequently.

It is not intended, however, to discuss either the structure or physiology of the air-bladder in Teleostean Fishes in general, except in so far as the facts already mentioned are relevant to the special subject of this paper, which is—the morphology, and more particularly the physiology, of the air-bladder in one of the five families in which the so-called "auditory" or Weberian ossicles are present, viz., the *Siluridæ*.

The *Siluridæ* constitute one of the largest families, if not actually the largest, of fresh-water Fishes, comprising considerably upwards of seven hundred species, referable to a large number of generic types. Their geographical distribution is almost coextensive with the tropical and sub-tropical regions of the earth's surface, and the habitat of the different species is characterized by remarkable variety, ranging from mountain streams often at an altitude of several thousands of feet above the sea level, through the larger rivers and lakes of the plains, to the open sea, and even to oceanic islands. Apart from their geographical distribution, the physical conditions under which many *Siluridæ* live are almost equally varied. Most of them are ground-feeders; some have exceptional powers of living out of their natural element; others are liable, through prolonged drought and the drying up of rivers, to become buried in the mud at the river bottom for longer or shorter periods without subsequent injury; and others, again, are said to be capable of migration upon land from one river or lake to another. In view of the exceptionally varied geographical and physical conditions to which the *Siluridæ* are subject, it becomes a matter of some interest to inquire how far such conditions are capable of influencing the structure of so specialized a mechanism as that which the internal ear, the Weberian ossicles, and the air-bladder combine to form in this large and most interesting family of Teleostean Fishes.

It will be necessary to refer briefly to what has already been done by previous writers on this subject. Weber (*l.c.*) was the first, in 1820, to show that in certain families of Physostomous Teleostei the air-bladder is connected with the internal ear by a chain of three moveably articulated "auditory" ossicles. Of the five families in which this mechanism is present the *Siluridæ* have received but comparatively scanty attention since the publication of Weber's classical memoir. Weber himself only described the air-bladder and "auditory" ossicles of one species—the only European species of the family —*Silurus glanis*. Johannes Müller (9) added considerably to our knowledge of these structures, and notably by his discovery of the "springfederapparat," but his attention was mainly directed to the grosser features in the anatomy of the air-bladder to the exclusion of all but the slightest reference to the important and highly characteristic skeletal modifications, which are associated with the peculiar structure of that organ in the *Siluridæ*. Reissner (10) has given a fairly complete account of the bone-encapsuled air-bladder of *Rhinelepis;* and brief, and more or less incomplete, accounts of this organ are to be found in the zoological writings of Dr. Taylor, of Edinburgh (11), and the late Dr. Francis Day (12, 13). Among the most valuable of recent contributions to this subject must be mentioned the papers by Professor Ramsay Wright, of Toronto, relating to the aberrant South American Siluroid *Hypothalmus marginatus* (14), and to the more normal North American species of *Amiurus catus* (15, 16). By his various papers on *Amiurus*, Professor Wright was not only the first to recognize the exact nature of the "complex vertebra" through his discovery that it is formed by the coalescence of the centra of the second and third vertebræ with the fourth vertebra, but the first also to give a complete and accurate account of the skeletal relations and attachments of the component fibres of the air-bladder in any one Siluroid. Among the papers dealing with the structure of the air-bladder in isolated species, mention may be made of a paper communicated by me to the Birmingham

Philosophical Society in 1888 (17), on "The Air-bladder in certain Siluroid Fishes." In this paper it was first demonstrated that an air-bladder and a series of Weberian ossicles are present in the South American genus *Cetopsis*—a Siluroid in which, until then, those structures were believed, on the authority of Johannes Müller (*l.c.*), to be entirely absent. So far the air-bladder and Weberian ossicles had only been described in a very limited number of isolated species, and no attempt had been made to institute a systematic and comparative examination in any very considerable number of genera and species. An attempt in this direction was first made by Bridge and Haddon in their paper, "The Air-bladder and 'Auditory' Ossicles in the Siluridæ" (Proc. Roy. Soc., 1889) (18), of which only a preliminary abstract has so far been published; and the purport of the present communication is to discuss how far the facts elucidated in our paper are able to throw any light on the vexed question of the physiology of the so-called "auditory" or Weberian ossicles.

Before dealing with the special physiology of the Weberian ossicles, it will be convenient to describe the general structure of the air-bladder, and the arrangement and relations of the Weberian ossicles in the *Siluridæ*.* As a matter of convenience in anatomical description, the Siluridæ may be subdivided into two groups—the *Siluridæ normales*, and the *S. abnormales*. In the former group the air-bladder and Weberian mechanism are well developed, but in the latter group both structures are more or less rudimentary. It must be pointed out, however, that this classification, based as it is upon so variable an organ as the air-bladder, is purely arbitrary and of no genetic value.

In all the *S. normales* the air-bladder consists of two portions—a posterior, which is subdivided internally into two lateral compartments by a vertical longitudinal septum—and

* For further details as to the anatomy of the air-bladder in this family, reference may be made to No. 18 in the annexed list of papers dealing with this subject.

an anterior, transversely arranged, and undivided compartment. The anterior chamber is partially separated from the two lateral compartments by a transverse septum, round the lateral edges of which, each lateral chamber freely communicates in front with the anterior chamber. The two lateral compartments forming the posterior half of the bladder are inelastic and inexpansible, and this is partly due to the existence of the longitudinal septum, and partly also in many *Siluridæ* to the more or less extensive development of secondary transverse septa in each compartment. The anterior chamber, on the contrary, is more or less elastic and expansible. The distensibility of the anterior chamber is, however, by no means uniform in all directions. The investment of its dorsal wall by the expanded transverse processes of the fourth and fifth vertebræ, and the coalesced centra of the complex and fifth vertebræ, and of its anterior wall by the decurved portion of the first-mentioned transverse process, with or without the aid of the inferior limb of the post-temporal, tend to limit all possibility of expansion on the part of the anterior and dorsal walls. Moreover, the rigidity imparted to the anterior, ventral, and posterior walls, by the skeletal attachments of the fibres forming the anterior and posterior pillars, also combines to render the anterior chamber absolutely inexpansible in any direction, except laterally, in a direction at right angles to its antero-posterior axis, any expansion or contraction in this direction being in all probability accompanied by an inward or outward bulging of the external skin (lateral cutaneous areas), with which each lateral wall of the chamber is in close contact. Apart from their different capacities for expansion, there is always a marked difference in size between the anterior and posterior divisions of the air-bladder, the internal capacity of the former being always considerably less than the combined capacities of the two lateral compartments. In brief, then, the air-bladder of a normal Siluroid Fish consists of a relatively small, anterior, elastic and distensible chamber, and of a posterior, relatively large, inelastic and indistensible portion.

The internal ear is also characterized by certain structural peculiarities which are correlated with the peculiar connection between the air-bladder and the internal ear by a chain of moveably connected ossicles. The two sacculi are connected by a transversely arranged ductus endolymphaticus, which lies in a groove across the floor of the cranial cavity. From the centre of the ductus endolymphaticus, a thin-walled, pear-shaped sinus endolymphaticus is given off, which projects backwards into an excavation in the substance of the basioccipital bone—the cavum sinus imparis. The latter is prolonged backwards beyond the sinus endolymphaticus into two small and somewhat spherical chambers, which lie in the floor of the neural canal over the centrum of the first vertebræ, and have partly bony and partly fibrous walls. The outer wall of each atrium is incomplete, and is closed only by the spatulate process of the most anteriorly situated of the three Weberian ossicles. It will follow from the relations of the outgrowths from the internal ear to the cavities in which they lie, that, whereas the former will be filled with endolymph, the latter will contain perilymph, which will also extend into and fill the atrial cavities and bathe the inner surfaces of the spatulate processes of the two scaphia.

The physiological connection between the internal ear and the air-bladder is brought about by a chain of three moveably connected ossicles on each side of the anterior portion of the vertebral column. Of these ossicles the most posterior, or tripus,* is partially imbedded in the corresponding half of the dorsal wall of the anterior chamber of the air-bladder, while the most anterior, or scaphus, forms the outer wall of the atrial cavity of its side. Each tripus is a tri-radiate ossicle with three characteristic processes, of which the hinder or crescentic process is imbedded in the dorsal wall of the anterior chamber

* For reasons given elsewhere (18), it has been thought advisable to substitute the names tripus, intercalarium, and scaphium, for the names malleus, incus, and stapes, originally given by Weber (*l.c.*) to the three Weberian ossicles.

in such a way that it receives the insertion of all the fibres which converge from the lateral wall of the chamber and extend into the dorsal wall. The second or articular process of the tripus fits into a socket on the side of the centrum of the third vertebra, while the third or anterior process extends forwards and is connected by an inter-ossicular ligament with the spatulate process of the scaphium. The last mentioned ossicle consists of a horizontal process directed forwards and expanding anteriorly into the spoon-shaped plate which forms the outer wall of the atrium of its side. At the hinder extremity of the horizontal or spatulate process a vertical process is given off, which terminates inferiorly in a rounded condyle, and fits into a socket on the dorsal surface of the centrum of the first vertebra. For the remainder of its extent the vertical process lies in the wall of the neural canal, which, in this region, is only fibrous. The inter-ossicular ligament is a short ligament composed of tendinous fibres and passing transversely from the anterior end of the tripus to the convex outer surface of the spatulate process of the scaphium. The third ossicle, or intercalarium, varies greatly in degree of development in different *Siluridæ*. In most species it is a mere bony nodule imbedded in the inter-ossicular ligament; in some, however, the intercalarium consists of two processes, an ascending process lying in the fibrous wall of the neural canal, and a horizontal process, which extends outwards and has its distal extremity inserted into the inter-ossicular ligament.

From the position and relations of the Weberian ossicles to the internal ear and to the air-bladder, it will follow that any inward or outward bulging of the lateral walls of the anterior chamber, which, it must be remembered, are the only portions of the air-bladder capable of expansion or contraction, will necessarily cause the crescentic process of the tripus to move inwards or outwards, while the anterior process of the same ossicle will execute similar movements, but in inverse order. In such movements the tripus represents a lever of the first order, the fulcrum of which is represented by the articulation

of the articular process with the side of the third vertebral centrum. The relative lengths of the two arms of the lever, *i.e.*, the crescentic and anterior processes, vary somewhat in different *Siluridæ*, although within rather narrow limits. Very exceptionally the anterior process is slightly the longer, and consequently, like the long arm of a lever, gains in amplitude what it looses in force. Movement, inwards or outwards, on the part of the anterior process will necessarily impart either a push or a pull to the spatulate process of the scaphium, which, therefore, moves inwards or outwards by a corresponding rotation of its condylar process; and with every such movement of the scaphium, a forward or backward flow of the perilymph of the atrial cavities must take place, with the final result that variations in the internal condition of the air-bladder become competent to act as stimuli to the sensory epithelium of the internal ear.

There is one point in connection with the inter-ossicular ligament which deserves some attention. Although of considerable thickness in proportion to its length, the inter-ossicular ligament must not be regarded as a rigid structure, but rather as being more or less compressible when subjected to pressure operating at right angles to the direction of the length of its component fibres. such for instance, as it seems to me, must take place when the anterior process of the tripus moves inwards towards the scaphium. Conversely, any slight outward movement of the latter process, which might subsequently take place, would, at first, only have the effect of straightening, as it were by a pull, the outwardly bulging fibres of the ligament. Hence it would seem that relatively slight incursions or excursions of the anterior process of the tripus can neither exert any influence on the scaphium, nor secondarily on the fluids of the atrial cavities; and that only such movements of the tripus as are of greater amplitude than those now under consideration can be regarded as competent to produce corresponding movements of the scaphium, and ultimately affect the sensory epithelium of the internal ear.

I cannot but regard the inter-ossicular ligament as fatal to all theories as to the function of the Weberian ossicles which rest on the assumption that the latter are capable of transmitting delicate or rapidly recurring vibrations from whatever source they may be derived. In fact, the comparative laxity of the inter-ossicular ligament seems almost as if it were a special contrivance to prevent the internal ear from becoming susceptible to any but the comparatively grosser changes which may take place in the internal condition of the air-bladder. It is difficult, indeed, to avoid the conclusion that the Weberian apparatus constitutes a mechanism which, while not sufficiently delicate as to be capable of transmitting to the internal ear slight variations in the condition of the gaseous contents of the air-bladder, is, nevertheless, very susceptible to all such changes that exceed a certain minimum.

A brief reference must also be made to the existence of an oblique valve projecting into the narrow lumen of the ductus sacculo-utricularis, that is, the duct by which the utricular and saccular divisions of the internal ear are placed in communication with each other. (16.)

It may be added that in nearly all *Siluridæ* there is a permanently open ductus pneumaticus, and also that the vascular plexuses, or retia mirabilia, so characteristic of other Teleostei, are always absent.

From the morphology of this singular mechanism we may next proceed to the discussion of its physiology, and the advantage which these Fishes derive from its possession.

An initial difficulty in the way of any attempt in this direction is the complex physiological character of the two organs—the air-bladder and the internal ear—which the Weberian mechanism brings into such close anatomical and physiological relations. The auditory organ is almost as diverse in its varied functions as in its modes of evolution. Primarily evolved as an organ for enabling its possessor to appreciate the grosser movements or vibrations—as distinguished from sound waves—which may take place in water, as in the case of the otocysts of the

Hydromedusæ, or the tentaculo-cysts of the Scyphomedusæ, the researches of Engelmann and Delarge (19, 20) render it extremely probable that the auditory organ in the higher Invertebrata may perform, singly or conjointly, the functions of equilibration and audition. In the higher Vertebrata, and probably also in the lower, as the often quoted experiments of Goltz appear to demonstrate, the auditory organ subserves both functions, the semi-circular canals being the seat of equilibration, while the remainder of the internal ear is devoted to the purpose of hearing. But as this double function could never have been detected by purely anatomical investigation, it is by no means certain that we have exhausted all the physiological possibilities of the so-called auditory organ in different organisms, or entirely excluded the possible existence of some as yet undiscovered function. Hence, there are at least two functions to be considered in connection with the internal ear, viz., hearing and equilibration.

The air-bladder in different Fishes is even more complex in function than the auditory organ—more so, possibly, than any other organ. Among the varied functions that have been attributed to the air-bladder by different writers, we may mention (I.) phonation or sound production, (II.) respiration, (III.) as accessory to audition, or (IV.) hydrostatic. As it is inconceivable that the internal condition of the air-bladder can be directly modified through the intervention of the Weberian mechanism by any change initiated in the fluids of the internal ear, the real problems to be solved are these (I.) to which of the various known functions of the internal ear and air-bladder are the Weberian ossicles to be regarded as accessory structures, and (II.) what advantage does the Fish derive therefrom? Certain of the functions of the internal ear and air-bladder to which reference has just been made may be speedily eliminated from discussion.

The function of equilibration may be at once dismissed from consideration. No conceivable changes are likely to take place in the internal condition of the air-bladder as the result

of any movements of oscillation or rotation on the part of the Fish, and without such changes no motion of the Weberian ossicles could take place, and hence no modification in the condition of the perilymph or endolymph of the internal ear would be produced. But even supposing that movements of oscillation or rotation were competent so to alter in some way the internal condition of the air-bladder or its gaseous contents, that increased pressure or a flow would be produced in the fluids of the atrial cavities, the cavum sinus imparis, and secondarily in the endolymph of the sinus endolymphaticus and sacculus, the valve in the ductus sacculo-utricularis would tend to hamper, even if it did not entirely prevent, the extension of such disturbances into the semi-circular canals where the function of equilibration is supposed to reside.

In addition to the various other methods by which sounds are produced by different Fishes, the air-bladder not infrequently shares in the function of sound production. It has been established by Moreau (*l.c.*) that the grunting sounds emitted by Gurnards are caused by the vibration of an annular diaphragm within the air-bladder. Such diaphragms exist in a more or less complete condition in several other Fishes, as, for example, in the John Dory (*Zeus faber*), which produces sounds analagous to those of Gurnards. Other Fishes, on the authority of Dufosse (7), are capable of producing sounds in the form of breathing noises (les bruits de souffle), either by the expulsion of gas from the air-bladder through the ductus pneumaticus, or from the alimentary canal per anum. In reference to the special case of the *Siluridæ*, very little is known either as to their capacity for sound production, or to the mechanism by which the sounds are made. A breathing noise is made by the only European species of Siluridæ (*Silurus glanis*), according to Dufosse (*l.c.*); and Sir Emerson Tennant* states that a species of *Clarias*, found in the lake at Colombo, is

* Instincts and Emotions in Fish, by Francis Day, Trans. Linn. Soc., Zool., vol. xv., 1880.

said by fishermen to make a grunting sound under water when disturbed. The African Siluroid (*Synodontis Arabi*) also makes a breathing noise, which Dufosse (*l.c.*) thinks is probably connected with the air-bladder. But in all these instances there is no definite evidence to show that the sounds produced are in any way connected with the air-bladder and not the result of the expulsion of gas per anum. In *Clarias*, at all events, it is almost certain that the grunting sound which it is said to make could not be made by the expulsion of gas from the air-bladder, inasmuch as the bladder is almost completely encapsuled by bone; and in other *Siluridæ* the absence of extrinsic muscles, in all but a very few genera, by which the expulsion of gas from the air-bladder could be effected, combined with the absence of internal vibratory diaphragms in all Fishes of this family, are quite sufficient to prove the subordinate and altogether secondary nature of sound production as a function of the air-bladder, at all events by any of the ordinary methods known in other Fishes. On these grounds it seems permissible to exclude phonation as a possible function of the air-bladder from any discussion as to the physiological significance of the Weberian ossicles.

With regard to respiration, the air-bladder may be accessory to this function in one of two ways. Jobert (*l.c.*) is of opinion that in certain Brazilian Fishes, viz., *Sudis gigas*, *Erythrinus tæniatus*, and *E. braziliensis*, the air-bladder serves directly as a lung subsidiary to the gills, since they inhale air through the ductus pneumaticus, the Fish dying of asphyxia when the duct is ligatured. But apart from this more direct method of breathing, there is a large body of experimental evidence for the belief that the air-bladder has an important although secondary relation to respiration, by acting as a reservoir for the superabundance of oxygen which is taken into the blood through the gills, and subsequently reabsorbed from the air-bladder into the blood and used up when the Fish is in water containing but little oxygen. It seems clear, nevertheless, that however important may be the secondary relation of the

air-bladder to the function of respiration, either in the *Siluridæ* or in Fishes in general, there is no physiological relation between this function and the existence of a Weberian mechanism. There is absolutely no evidence of direct respiration by means of the ductus pneumaticus; and the function of the air-bladder, as an oxygen reservoir is, to say the least, as characteristic of those Fishes in which a Weberian mechanism is absent as of those in which it is present. In fact, the interesting and valuable experiments of Moreau (*l.c.*) prove to the point of actual demonstration that of the Fishes with which he experimented those provided with a Weberian mechanism had scarcely any capacity for absorbing oxygen from the air-bladder when placed under conditions calculated to produce asphyxia, *i.e.*. in water deprived of its oxygen; and, without doubt, this deficiency may be associated with the absence of the special retia mirabilia, upon the existence of which any marked capacity for gaseous absorption seems to depend.

Dismissing the respiratory function of the air-bladder from all connection with the Weberian mechanism, we may next consider whether the air-bladder and Weberian ossicles may be regarded as structures accessory to the function of hearing.

Weber himself not only regarded the ossicles which bear his name as homologous with the incus, malleus, and stapes of the Mammalian ear, but also attributed to them an exactly similar function. Weber imagined that sound waves impinging on the general surface of the body ultimately affected the air contained in the air-bladder, and the latter, acting as a resonator, transmitted by means of the Weberian ossicles the vibrations to the internal ear. Presumably, the only way in which sound waves traversing the water in the vicinity of the Fish could affect the Weberian ossicles and reach the internal ear would be by traversing the body wall of the animal and throwing into vibration the air contained in the air-bladder; such vibrations impinging upon the only portion of the air-bladder at all likely to be capable of vibratory movement, viz., the lateral walls of the anterior chamber, might possibly

initiate corresponding vibratory movements in the chain of Weberian ossicles, and ultimately affect the fluids and sensory epithelium of the internal ear.

Apart from the *a priori* probability that the existence of an anatomical connection between the internal ear and the air-bladder is for the purpose of rendering the latter organ a physiological accessory to the function of hearing, the following facts are by no means inconsistent with Weber's theory :—

(1) The existence of lateral cutaneous areas in close relation with the lateral walls of the anterior chamber of the air-bladder. Such areas would undoubtedly offer greater facilities for the passage of sound vibrations from the external medium to the air contained in the air-bladder than if such vibrations had to be transmitted through the musculature of the body wall and the intervening viscera, which would certainly be the case if such areas had no existence.

(2) The relatively small size of the eyes in the great majority of the Siluridæ. This fact suggests the inference that defective vision might not unreasonably be associated with increased power of hearing.

(3) The power of sound production which exists in certain Siluridæ. A capacity for making sounds might not improbably be accompanied by an increased capacity for hearing them.

On the other hand, there are many and weighty reasons for the opinion first expressed by Hasse (25), that Weber's theory is untenable, and of these we may direct attention to the following :—

(1) Granting that there are certain facts relating to the size and structure of the eyes and the muddy water habitation of the *Siluridæ* which would render increased power of hearing of great advantage to the Fish, it may be pointed out that an increase in the acuteness of the sense of hearing may be brought about by other means than the specialization of the air-bladder for that purpose. The transmission of sound vibrations from the external medium to the internal ear is undoubtedly facilitated in many *Siluridæ* by the extreme

thinness, amounting in some cases almost to transparency, of certain bones by which the ear is enclosed and protected. The medio-dorsal vacuity (posterior cranial fontanelle) between the frontal bones of the skull, which is closed only by the scaleless skin and fibrous tissue, may also afford an easy path by which sound waves could reach the perilymph-infiltrated tissue of the cranial cavity, and eventually the perilymph surrounding the internal ear.

(2) The existence of lateral cutaneous areas might certainly be supposed to facilitate the passage of sound vibrations from the exterior to the air in the air-bladder, but their significance in this respect is seriously lessened by the fact that they are confined to the *Siluridæ*, and altogether absent in the other families in which a Weberian mechanism is present.

(3) The very limited number of *Siluridæ* known to possess organs of sound production is a very forcible argument against attaching too much importance to the possible existence of increased powers of hearing in this family, at all events so far as the Weberian ossicles are concerned.

(4) The inertia of the structures concerned in the transmission of vibrations from the air-bladder to the internal ear. The extreme thickness of the walls of the air-bladder in many, if not in most, *Siluridæ* seems to me to be somewhat inconsistent with the supposition that they can be susceptible to ordinary sound vibrations affecting the gaseous contents which they enclose, and this inconsistency is emphasized in certain Siluridæ, such as *Osteogeniosus*, *Ælurichthys*, and some species of *Arius*, by the development of vertically-placed buttress-like aggregations of the fibres composing the inner fibrous stratum of the lateral walls of the anterior chamber. The size and thickness of the Weberian ossicles in many Siluroids, the somewhat rigid articulation of the articular process of the tripus with the third vertebral centrum, which in some instances is effected by the actual continuity of that process with the centrum in question, seem to point to the conclusion that the Weberian mechanism is better adapted to register the more forcible, even if more

gradual, distensions and contractions of the anterior chamber, rather than the relatively slight but more rapidly recurring vibrations of the lateral walls, which, conjecturally, might be produced by the impact of sound waves. Moreover, both the structure and relations of the inter-ossicular ligament to the tripus and scaphium afford, as it seems to me, a fatal objection to the theory that the Weberian ossicles as a whole can vibrate synchronously with rapidly recurring sound waves. In the, in some respects, analagous mechanism of the Mammalian tympanum, the auditory ossicles are in no instance separated by a ligament of any description, and the different ossicles directly articulate with one another, and hence are capable of vibrating as a rigid whole synchronously with the vibrations of the tympanic membrane, and consequently of communicating such vibrations to the perilymph of the internal ear. The want of rigidity in the inter-ossicular ligament, therefore, seems to be an insuperable barrier to the transmission of sound vibrations from the air-bladder to the internal ear by means of the chain of Weberian ossicles, although it offers no obstacle to the transmission of any changes in the condition of the anterior chamber which might result in the more extensive inward or outward bulging of its lateral walls by whatever cause produced.

(5) Even if it be admitted that the air-bladder and its ossicles are accessory to hearing, the Fish could have no appreciation of the *direction* of sounds conveyed to the internal ear by that organ. All cognizance of the varying directions from which sounds may come is generally believed to be due in all Vertebrated animals to the differential action of the two ears; but in all Fishes in which a Weberian mechanism is present, any vibrations that may be transmitted from the air-bladder, in accordance with Weber's theory, must, in the first instance, be communicated to the perilymph of the atrial cavities and to the unpaired sinus endolymphaticus, finally reaching each internal ear by the ductus endolymphaticus. Hence, it necessarily follows that both internal ears will be affected at the same moment and to an equal extent, and no differential action of

the two ears can possibly take place. It would seem, therefore, that any increased acuteness of hearing that might be conferred upon the Fish by the air-bladder in its presumptive function of an accessory to hearing would be counterbalanced by the fact that such increase of auditory capacity could not be associated with a corresponding increase in the power of appreciating the direction of sounds. Whatever may be the physiological importance of the sense of hearing in Fishes, whether associated with the pursuit of prey, or as an aid in escaping from enemies, or, as in the case of gregarious fishes, as a means of keeping together in shoals, either for breeding or for other purposes, the power of appreciating the direction of sounds must be of primary importance in any modification of the auditory organ in the direction of giving to its owner exceptional powers of hearing.

It may be concluded, therefore, that if the physiology of hearing in Fishes is in all essential respects similar to that of the higher air-breathing Vertebrata, bearing in mind the differences in the medium in which they live as regard conductivity for sound, then the air-bladder and Weberian ossicles cannot be regarded as subsidiary to the function of hearing, and the solution of our problem must be looked for in some other direction.

Dismissing sound production, equilibration, respiration, and audition from all physiological connection with the existence of Weberian ossicles, we may next consider the sole remaining alternative view—that the ossicles in question are accessory to the hydrostatic function of the air-bladder. But before dealing with the function of the Weberian ossicles in this connection, it will be advisable to state what is accurately and experimentally known of the hydrostatic function of the air-bladder in Fishes in general.

One of the earliest theories of the hydrostatic function of the air-bladder with which I am acquainted is that propounded by Borelli (21), in 1680, and subsequently adopted without any attempt at experimental proof by almost every writer on this subject until the publication of Moreau's paper in 1876. Borelli stated his theory in the following words: "The Fish is

a machine resembling a cylinder and containing within its abdomen a vessel full of air, which, in the mean condition, is sufficiently compressed to render its density exactly equal to that of the water which surrounds it, so that the immersed Fish can rest absolutely immotile. But if it wishes to become lighter, like a self-elevating machine, the Fish must relax its muscles, in order to allow the air to obey its elastic force and dilate; on the contrary, if the Fish wishes to become heavier and descend even to the bottom, it is necessary that it should contract its air-vessel by means of its abdominal muscles, in order to occupy a smaller volume." According to this view the function of the air-bladder, in its mean condition, is to render the specific gravity of the Fish equal to that of the water in which it lives, and also to aid the Fish in its movements of ascent and descent in the water, such movements being due either to the increased specific gravity of the animal, as the result of muscular compression of the air-bladder, or to the diminished specific gravity which would result from muscular relaxation and the consequent expansion of the contained gases.

From the time of Borelli until the publication of the results of Moreau's experimental researches in 1876, the function of the air-bladder proved a fruitful source of discussion amongst zoologists and comparative physiologists, of whom the names of Cuvier, Delaroche, Stannius, Gouriet, Owen Biot, Monnoyer, and Johannes Müller may be mentioned. Most of these writers, however, adopted the theory of Borelli, with scarcely any modification, and without any attempt at experimental proof.

Johannes Müller (*l.c.*), while accepting Borelli's theory for Fishes in general, is responsible for another view as to the hydrostatic function of the air-bladder in certain families, which has been widely held, but, like Borelli's theory, has never received the sanction of experimental proof. Müller's theory is—that those Fishes, as for example many Cyprinidæ, in which the air-bladder is partially constricted into an anterior and a

posterior sac, possess the power, by muscular contraction, of driving the air from one chamber to the other, and thus producing a displacement of the centre of gravity upon the longitudinal axis of the animal. By this means such Fishes were supposed by Müller to be able, by see-saw movements, to vary in a vertical direction the long axis of the body, the fins then causing the Fish to advance in the new direction which the axis of its body had taken.

Monnoyer (22), on purely theoretical grounds, also adopted Müller's theory, but at the same time admitted his inability to support his conclusions by any experimental evidence.

More recently the theories of Borelli and Johannes Müller have been successfully opposed on experimental grounds, first by Moreau (*l.c.*) in 1876, and secondly by Charbonnel-Salle (23) in 1887.

Summarising the results of the investigations of these two writers, it seems to me that the following deductions as to the hydrostatic function of the air-bladder in Fishes in general may be legitimately drawn from them :—

(1).—That the function of the air-bladder in Fishes is to render the Fish, bulk for bulk, of the same weight as the medium in which it lives. In this mean condition, or plane of least effort, as Moreau terms it, the Fish acquires a capacity for the maximum amount of locomotion with a minimum of muscular effort.

(2).—In its movements of ascent and descent, the Fish becomes exposed to augmented or diminished pressure, which in each case varies in amount, according to the variable height of the superimposed column of water, and this leads to an expansion or contraction of the air contained in the air-bladder, and therefore to an increase or diminution in the volume of the Fish, and thereby to a corresponding alteration in its specific gravity, which may temporarily remove the animal from its normal plane of least effort.

(3).—The Fish has no power of varying the capacity of its air-bladder by direct muscular contraction, and its adjustment

to a new plane of least effort results from a gradual increase or decrease in the volume of the air contained in the air-bladder to an extent proportional to the new pressure, and due to a corresponding modification of the processes concerned in the secretion or absorption of the contained gases. Hence, by this apparently automatic method of adjustment, the Fish will find, sooner or later, and whatever may be the depth of the water and the amount of external hydrostatic or atmospheric pressure, a plane of least effort, where it will possess exactly the density of the surrounding medium.

(4).—That Johannes Müller's theory of the displacement of the centre of gravity upon a longitudinal axis in the case of Fishes with two-chambered air-bladders has no foundation in fact.

The general drift of Moreau's conclusions may perhaps be rendered clearer by a simple illustration. A Fish, possessing a normal volume and occupying a plane of least effort in which the specific gravity of its body is equal to that of the water in which it lives, moves from a less to a greater depth, and thereby becomes exposed to increased hydrostatic pressure. The increase of pressure by diminishing the volume of gas contained in the air-bladder and also, at the same time, the bulk of the Fish, will give to the body of the Fish a specific gravity greater than that of the water. To counteract this increase of specific gravity and restore the Fish to a normal volume and a fresh plane of least effort at the new level, an augmentation in the amount and volume of gas present in the air-bladder takes place by a process of secretion from the blood. Reciprocally, the Fish in rising from a greater to a less depth becomes exposed to diminished hydrostatic pressure, and the volume of gas in the air-bladder consequently expands and to a proportionate extent the specific gravity of the animal is reduced, but the normal volume, both of the Fish and the air-bladder, is again restored by a corresponding absorption of gas from the air-bladder into the blood, and the Fish once more acquires a plane of least effort at its new and more superficial

level. According to Moreau, by such means, a Fish provided with an air-bladder will always find, whatever may be the depth of the water, a plane in which it will possess exactly the density of the surrounding medium.

Although an air-bladder is an undoubted advantage to the generality of Teleostean Fishes, there can be no question that its presence is attended by certain disadvantages which, to a greater or less extent, limit and control the locomotor activities of its possessor. The air-bladder is an obstacle to rapid movements of ascent or descent, and tends to restrict the vertical range of locomotion on the part of the Fish within certain tolerably definite limits. As Moreau says (*l.c.*), when the Fish is in a plane of least effort the possession of an air-bladder is a distinct advantage; but for rapid changes of level, measured vertically, the air bladder may prove a considerable disadvantage or even a danger to the Fish. In a Fish exposed to artificially diminished pressure the air contained in the air-bladder will tend to assume and retain an augmented volume, and so lessen the specific gravity of the animal that it rises to the surface and can only rest with part of its body exposed to the air, and this helpless condition is retained until the partial absorption of the contained air restores the Fish to its normal density at the new level, when it again acquires its accustomed liberty of movement. The Fish, says Moreau, incurs more danger by rising above its plane of equilibrium than by sinking below this plane for the same vertical distance. This last statement may be illustrated by a quotation from Semper (24) with regard to the "Kilch" (*Coregonus hiemalis*)—a fish which inhabits the Lake of Constance. "These Fishes are caught in nets and brought to the surface of the water; they come up invariably with the belly much distended; the air in the swimming bladder, being relieved from the pressure of the column of water, has expanded greatly and occasioned this unnatural distension, which renders the fish quite incapable of swimming. Under these conditions the fish is naturally unable to live for any length of time. But

the fishermen of the lake have a very simple remedy; they prick into the air-bladder with a fine needle; the air escapes with some force, the distension subsides, and the fishes are enabled to live under totally changed conditions as to pressure, even in quite shallow water and at the surface, swimming as freely as their companions, the natives of the surface water. Hence, the Kilch is confined to a certain depth because it is not capable of accommodating the tension of its swimming bladder to the change of pressure in the column of superincumbent water." A further illustration of the same point is afforded by the behaviour of certain deep-sea Fishes when forcibly drawn or carried to the surface from great depths, both the air-bladder and body being enormously distended, the former not infrequently ruptured, while there is usually, in addition, a prolapsus from the mouth or anus of the contiguous portions of the alimentary canal.

The restriction of the vertical range of the Fish is clearly due to the slowness with which the secretion or absorption of gases into or from the air-bladder takes place, and the facts above quoted are in complete accord with the experimental observations of Moreau, from which it would appear that the adjustment of the volume of gas contained in the air-bladder to varying pressures is a comparatively slow process, the length of which, however, varies in different Fishes and under different conditions from a few hours to several days. It is, perhaps, not unreasonable to suppose that a Fish, while under normal conditions and free in the water, is liable to accidents similar in kind, if less in degree, to those caused artifically by experiment, whenever it ventured upon rapid vertical movements involving considerable changes of level, and of such movements that ascent is likely to prove much more dangerous than an equal distance of descent. Slow or leisurely movements in a vertical direction, extending over periods of a few hours, may be met by a corresponding readjustment of the volume of gas contained in the air-bladder, so that whatever may be the new level, the Fish still retains its plane of least effort; but in rapid and con-

siderable changes of level this readjustment cannot be so easily or so readily brought about. Hence, in the generality of Teleostean Fishes, and more particularly, perhaps, in the Physoclisti, the air-bladder restricts freedom of movement in a vertical direction, and confines ordinary locomotion within more or less well defined vertical limits above or below the tolerably constant mean position in which the special advantage of an air-bladder is best realized.

Upon the basis of the conclusions established by Moreau and Charbonnel-Salle, it is obvious that the varying degrees of tension of the gaseous contents of the air-bladder, due to variations in the height of a superincumbent column of water, constitute an important factor in the physiology of locomotion in those Fishes in which that organ is present; and hence, in the absence of any other tenable hypothesis as to their function, there is a strong antecedent probability that the object of the Weberian ossicles is to acquaint the Fish with the varying degrees of tension to which the air-bladder may be subjected, and thereby, in some way or other, to facilitate the adjustment of its locomotor activities to variations in the physical conditions of its external environment. Hasse (26), who was the first to oppose the then prevalent idea of the air-bladder and Weberian ossicles being accessory to audition, without however entirely excluding the possibility that such might be the case in some subordinate degree, was also the first to suggest that the object of the mechanism was to bring directly to the consciousness of the Fish the varying tensions of the gases contained in the air-bladder. It is clear, however, that the Fish is not only exposed to varying hydrostatic pressures according to the depth at which it lives, but also, at the same time, to similar variations of atmospheric pressure as measured by the barometer; but, according to Hasse's view, it is the perception of hydrostatic pressure with which the Weberian apparatus is more directly concerned. The late Dr. Sagemehl (*l.c.*) also adopted Hasse's view, at least so far as to regard the Weberian apparatus as constituting a register of pressure variations, but with this

important modification, that it is not hydrostatic but atmospheric pressure which the Fish is thereby enabled to appreciate. Postponing for the present any further reference to Hasse's theory, it becomes necessary to ascertain what measure of truth there may be in Sagemehl's barometrical theory. A brief statement of this ingenious theory may be given in Dr. Sagemehl's own words: "It may here be permitted me to remark that I have been led to the result through a whole series of facts that the pressure oscillations of the surrounding medium, which the Fish can appreciate by means of the Weberian apparatus, are less the oscillations of a superincumbent column of water than atmospheric pressure oscillations. In a word, that the Weberian apparatus is not present in order to indicate to the Fish the depth at which it is situated, but is, in the first place, a contrivance which informs the Fish of atmospheric variations and consequent impending changes in the weather."

It is very difficult to feel that the arguments by which Sagemehl supports his barometrical theory of the function of the Weberian mechanism are conclusive or even weighty. The anatomical features in the structure of the air-bladder upon which he lays considerable stress are, to say the least, quite as well adapted to register variations of hydrostatic pressure as variations of atmospheric pressure. Apparently, the only special reason that Sagemehl assigns in support of his barometrical theory is, that most of the Fishes possessing a Weberian mechanism are "ground" forms, in which the air-bladder, as a purely hydrostatic apparatus, is but of trifling significance, and, therefore, that the Weberian apparatus, as a register of varying hydrostatic pressure, is altogether superfluous. But even this reason is scarcely a cogent one. Whether the particular Fishes referred to are to be regarded as "ground Fishes" or not depends entirely upon the precise sense in which the term is applied to them. The majority of them are certainly not "ground" Fishes in the same sense as are, for example, the Skates and Rays or the Flat Fishes (Pleuronectidæ), which, when not moving by the exercise

of their fins, habitually rest on the sea bottom; but, on the contrary, live near but not on the bottom, and an air-bladder, as a hydrostatic organ, is therefore as useful to them as it undoubtedly is to Fishes which more habitually live nearer to the surface. In those *Cyprinidæ* and *Siluridæ* which are more strictly "ground" Fishes, as for example the Loaches (*Nemachilus*, *Cobitis*, *Botia*, and others), in the former family, and such genera as *Glyptosternum*, *Englyptosternum*, *Amblyceps*, *Exostoma*, *Pseudechineis*, and other genera belonging to rapidly flowing or alpine rivers in the latter, the air-bladder undergoes degeneration in both size and structure, just as it does in many other Fishes (*e.g.*, Pleuronectidæ), with a precisely similar habitat, and, therefore, can scarcely function as a register either of barometrical or hydrostatic pressure variations. But there are other and more salient reasons which may be urged in opposition to Sagemehl's theory.

Variations of atmospheric pressure are after all not very great. A variation of pressure sufficient to raise or depress a column of mercury in a barometer to the extent of half an inch, can have but a very trifling effect on an air-bladder, already, it may be, subjected to a very considerable hydrostatic pressure. A variation of half an inch implies an increase or decrease of pressure on the Fish to the extent of ·2456lb. per square inch at 62° F., but as hydrostatic pressure varies to the extent of ·432785lb. per square inch for every foot of ascent or descent in the water, and as a Fish at a depth of, say, six feet, is already subjected to a hydrostatic pressure of 2·5967lb. per square inch, it will therefore follow that an increase or decrease of half an inch of atmospheric pressure will only involve a variation of pressure amounting to less than one-tenth of the already existing hydrostatic pressure. Then again, as mercury is 13·6 times heavier than water, a variation of half an inch in a column of mercury would be about equal to somewhat less than seven inches of water, and hence the ascent or descent of the Fish to the trifling extent of seven inches would entirely mask any variation of atmospheric pressure to the amount indicated

by half an inch of rise or fall in the barometer. Moreover, a barometrical variation of even half an inch takes place but slowly, and rarely occurs in less time than several hours, and, consequently could only be appreciated, as distinct from hydrostatic pressure, on the somewhat improbable supposition that the Fish remained at exactly the same depth in the water during the whole time that the barometrical variation was in progress. The maximum range of atmospheric pressure variation, as measured by the barometer, is somewhere about two inches, but variations to this extreme extent occur only at considerable intervals of time, and even then may take hours to accomplish. The maximum amount of variation in atmospheric pressure to which a Fish is liable to be subjected, would, therefore, be equivalent to a variation of depth amounting to twenty-seven inches. But even in this extreme case the atmospheric pressure variation might be completely negatived by a corresponding rise or fall of the Fish in the water in the ordinary course of its locomotor movements, or, at all events, might be more or less effectually masked by even less extensive movements of ascent or descent, unless the somewhat improbable supposition be entertained that it is the constant habit of these Fishes to remain at almost precisely the same level in the water for many hours together. Hence, it seems very difficult to avoid the conclusion that the function of the Weberian mechanism, as a register of atmospheric pressure variations, would be rendered altogether futile by the more marked variations of hydrostatic pressure—due in a limited number of instances, as in marine or brackish water *Siluridæ*, to the ordinary rise or fall of the tide, or much more frequently, as in fresh-water *Siluridæ* or other Ostariophyseæ, to movements of ascent or descent of comparatively trifling extent. Even if the truth of Sagemehl's theory be admitted, it is still very difficult to imagine what advantage these Fishes could derive from their ability to appreciate variations in the pressure of the atmosphere and consequent impending changes in the weather, nor is it easy to conceive in what way the Fish can adjust its

habits, or its locomotor activities, in accordance with varying meteorological conditions. It might possibly be advantageous for certain marine species of active habit to be able to anticipate a storm and retreat to deeper and calmer waters, but even this advantage could only apply to very few of the Fishes in which a Weberian mechanism is present, and would be altogether superfluous in the case of the fresh-water species, which form by far the greater majority of the four or five families concerned.

If we regard Sagemehl's theory as untenable, there remains only Hasse's view that the function of the Weberian ossicles is to acquaint the Fish with the varying hydrostatic pressures to which it is liable, or in other words that the Weberian mechanism is a register of hydrostatic pressure. Seeing that the primary and most important function of the air-bladder in all Fishes is that of acting as a float, there is a strong antecedent probability that the Weberian ossicles are physiologically related to this function. The general structure of the air-bladder, the relations of the Weberian ossicles, both to the internal ear and to the bladder, are perfectly consistent with this view, while, as I have already pointed out, such facts are absolutely inconsistent with any theory that depends upon the capacity of the air-bladder and Weberian ossicles for transmitting small or rapidly-recurring vibrations of the gaseous contents of the bladder, such as, for instance, must be transmitted if we are to accept Weber's theory that the structures in question are accessory to hearing. The general structure of the air-bladder, and especially its division into a small, elastic, and expansible chamber, and a relatively larger, inelastic, and inexpansible chamber, renders it admirably adapted for the measurement of the varying volumes of the contained gases. Regarded, in fact, as an article of physical apparatus for the measurement of its expanding or contracting gaseous contents under the influence of varying hydrostatic pressures, the air-bladder may be compared with such physical instruments as, for example, the thermometer, by which the expansion or contraction of mercury or alcohol under the

influence of changes of temperature is accurately measured. In both instruments there is a relatively large reservoir in free communication with a much smaller one, and the expansion of the relatively large mass of the gas or fluid, as the case may be, in the large chamber, is measured by the more readily observed expansion of the gas or fluid of the small chamber; and hence there is a close functional parallelism between the anterior and posterior chambers of the air-bladder and the capillary tube and bulb of a thermometer respectively. Similarly, it may be pointed out that the anterior chamber of the air-bladder is, to some extent, constructed on the principle of the aneroid barometer, inasmuch as the susceptibilty of the recording lever, that is the Weberian ossicles, in connection with the former to the varying volumes of the contained gases, is increased by the rigidity of its anterior, posterior, dorsal, and ventral walls, and the restriction of all expansion or contraction to movements of its lateral walls, in much the same way that the expansion or contraction of the air contained within the metallic box of the aneroid find their sole expression in corresponding movements of the flexible upper wall of the metallic box, which alone is capable of transmitting such internal volumetric changes to the recording lever. In fact, in a certain sense, it may be said that, considered as a piece of physical apparatus, adapted for the measurement of the varying volumes of its contained gases, the air-bladder combines the principles of both the thermometer and the aneroid barometer.

The conclusion to which these facts seem irresistibly to point is still further strengthened by the fact that in a large number of *Siluridæ* (*S. abnormales*) the assumption of a purely ground habit is almost invariably attended by the more or less complete degeneration of the air-bladder and Weberian mechanism. In all purely "ground Fishes" the air-bladder is a useless, and perhaps even a harmful, structure, and hence its liability to degenerate to the condition of a more or less rudimentary structure. Even in such cases where the air-bladder of these *Siluridæ* retains much of its structural integrity, but is

reduced in size to a relatively diminutive condition, the organ is so completely encapsuled by bone as to be entirely unable to alter its volume, at all events in the way of expansion, in response to variations in the hydrostatic pressure to which the Fish is subject. As an accessory to audition, or as a register of varying atmospheric pressures, the air-bladder might still be of some utility to such Fishes, notwithstanding their changed habit of life; but considered in relation to its normal function as a hydrostatic organ in Fishes in general, it would in all probability be absolutely valueless to them, and therefore it seems reasonable to infer that both the air-bladder and the Weberian mechanism are related solely to the primary hydrostatic function of the former.

To the first of the two initial questions with which this discussion commenced the following reply may now be given:—The function of the Weberian mechanism is to acquaint the Fish with the varying hydrostatic pressures to which it is liable in the course of locomotor movements involving ascent or descent in the water in which it lives. The second of the two questions —What advantage does the Fish derive from the possession of a pressure register, by which it becomes acquainted with the varying degrees of hydrostatic pressure to which it is liable? or in what way does the animal respond to the sensory impulses communicated to its central nervous system by the Weberian apparatus?—is one to which it is very difficult to return a satisfactory answer. No experimental enquiry has yet been made into the physiology of this curious mechanism, and while we desire to emphasize the danger of making physiological deductions from purely anatomical facts, no other course is at present open to us, and any conclusions based upon such data must partake rather of the nature of suggestions for future enquiry, and be accepted with considerable reserve. I cannot do much more than indicate the directions in which the solution of this problem must be sought.

It has already been pointed out that the researches of Moreau and Charbonnel-Salle warrant the belief that in Fishes

in general the function of the air-bladder is to give to the body of the Fish, in any mean position, the same density as the water, the variation in the amount of internal gas thus rendered necessary being due to the secretion or absorption of gas into or from the air-bladder, as the case may be; by this means the Fish will always acquire a plane of least effort at varying depths in the water. Now, this power of increasing or diminishing the volume of gas contained in the air-bladder appears to be very closely related to the presence of retia mirabilia in the walls of the air-bladder. Moreau found that those Fishes in which retia mirabilia were absent had scarcely any capacity whatever, either for secreting gas into the air-bladder or for reabsorbing gas from the air-bladder into the blood, and what little power in this direction they did possess was exercised with extreme slowness. This point has already been illustrated by reference to the brilliant experimental researches of Moreau (*l.c.*), wherein it was demonstrated that when the Carp and Perch were placed under conditions involving partial asphyxiation, the former possessed scarcely any power of reabsorbing the oxygen contained in the air-bladder, while the latter lost almost every trace of oxygen by that means; and also that this difference in absorptive power was due to the absence of retia mirabilia in the Carp and their presence in the Perch. Conversely, it was also shown by the same series of experiments that the capacity of the Fish for gaseous secretion from the blood into the air-bladder varied in precisely the same way and for exactly the same reason. All Fishes in which a Weberian mechanism is present are also characterized by the absence of retia mirabilia, and for this reason it seems clear that no adjustment of the volume of gas contained in the air-bladder, in accordance with varying hydrostatic pressures to which the animal is liable, can possibly take place as the result of gaseous absorption; and if the Fish possesses any power of bringing about such an adjustment, it must be by some other means than the absorption or secretion of gas from or into the air-bladder. On the other hand, all Fishes in which a Weberian mechanism is present belong to the

subdivision of Teleostei. termed Physostomi, a group in which a permanently open ductus pneumaticus connecting the air-bladder with the œsophagus, and, therefore, with the exterior, is present in the great majority of the species; and this fact combined, as I have already indicated, with the absence of retia mirabilia, at once suggests the possible inference that the adjustment of the volume of gas contained in the air-bladder to diminished hydrostatic pressure, may be affected not by the absorption of gas, but by the mechanical liberation of gas through the ductus pneumaticus and mouth. It is easy to see. in view of the greater danger to which the Fish is liable from over-distension of the air-bladder, that it would be of great advantage to the Fish to be so able to regulate the amount of air set free from the air-bladder by this means, that in gradually rising from deep water to more superficial levels the Fish would always occupy a normal plane of least effort, and retain perfect freedom of movement at all points in the course of its ascent. Two experiments of Moreau are extremely interesting in connection with this point. In one experiment a Perch was placed in a vessel of water under an air-pump, and as the air was gradually exhausted the Fish was irresistibly carried to the surface of the water and there remained perfectly helpless, unless by rupture the air-bladder forcibly gave exit to the expanded and imprisoned gases. In the other experiment a Tench was similarly treated, but, instead of being carried to the surface of the water, gave out through its mouth bubbles of gas at intervals, and was still able to swim about freely. The Tench is provided with a Weberian mechanism, a ductus pneumaticus, but has no retia mirabilia, and consequently the adjustment of its air-bladder to a diminishing pressure was brought about by the mechanical liberation of the necessary amount of gas through the ductus pneumaticus. The Perch, on the other hand, is devoid of a Weberian mechanism and a ductus pneumaticus, but possesses retia mirabilia; but, in the experiment referred to, the absorption of gas from the air-bladder took place too slowly to admit of adjustment to the rapidly

diminishing pressure, and hence the fish was helplessly carried to the surface of the water, where it was found to remain until such time as by the relatively slow process of gaseous absorption the air-bladder regained its normal volume under the new conditions, and the Fish again acquired a new plane of least effort.

It may therefore be suggested, as a tentative hypothesis, that the Weberian mechanism is a register of varying hydrostatic pressures, and that in response to the afferent impulses which are thereby communicated to the central nervous system, the Fish is enabled so to regulate the amount of gas liberated from the air-bladder through the ductus pneumaticus and the mouth, that, in all movements of ascent involving diminished hydrostatic pressure, the animal retains a plane of least effort at all points in its upward course, in which its specific gravity is approximately equal to that of the surrounding medium. The advantage of a reflex mechanism of this kind to the Fish is very obvious, inasmuch as the animal would possess greater freedom of movement in a vertical direction, notwithstanding the very considerable variations of hydrostatic pressure to which it must be subjected, and that this is the case, is to some extent, borne out by actual observation of the movements and habits of these Fishes.

The principal difficulty which this suggestion involves is that there is no evidence at present known as to how or in what way the escape of air through the ductus pneumaticus is regulated. That the escape of air is in some way regulated is rendered probable by the intermittent way in which air is liberated from the ductus pneumaticus when the Fish is exposed to artificially diminished pressure. In the case of *Caranx trachurus*, the exposure of the Fish to a gradually diminishing pressure led to the escape of air from the air-bladder through the safety canal (or canal de surété, as Moreau calls it) in a continuous stream; but with the Tench, under similar conditions, the escape of air through the ductus pneumaticus took place intermittently. These experiments

certainly suggest the existence in the Tench of some obstacle to what would otherwise be a gradual and continuous escape of air from the air-bladder, when the Fish was exposed to a gradual reduction of the external pressure, and it is possible that this obstacle may be of the nature of some regulatory mechanism in the ductus pneumaticus. Valves and sphincter muscles certainly exist in the ductus pneumaticus of some of these Fishes, and it may be conjectured that such structures are under the reflex control of some nervous centre, the activity of which may be excited or modified in response to afferent stimuli conveyed to the centre through the Weberian mechanism, the internal ear, and those special branches of the auditory nerve which are distributed to the sensory epithelium of the two sacculi. It may even be possible that, apart from the reflex control which is exercised over the action of the regulatory mechanism by the special nerve centre, the latter may be also dominated by some still higher centre, and the mechanism brought under the voluntary control of the animal.

The final solution of this problem must rest with the experimental physiologist, and I shall be satisfied if my own researches have furnished for this purpose a sufficiently wide and accurate basis of morphological data.

So far, the discussion has been restricted to the function of the Weberian mechanism in the *Siluridæ normales*, that is, those forms in which the air-bladder is well developed, and subdivided internally into three intercommunicating compartments, in the way already described in an early part of this paper, and in which also both the air-bladder and Weberian ossicles may be functionally regarded as constituting an apparatus for the perception of varying hydrostatic pressures. In all the *Siluridæ abnormales*, on the contrary, the air-bladder is always small, sometimes even diminutive, in proportion to the size of the Fish. Very generally also the air-bladder is more or less degenerate in other respects, sometimes having its cavity more or less completely obliterated by the partial solidification of the organ ; but, as a general rule, the air-bladder

is only represented by two very small laterally-situated air-sacs, which may intercommunicate or be entirely separated from each other, and together may be taken as representing the anterior chamber of the air-bladder of a normal Siluroid. Lateral compartments, as a rule, are absent altogether, or, if present, are extremely rudimentary. Whatever its condition in these *Siluridæ*, the air-bladder is almost always more or less completely enclosed within transversely-disposed bony recesses formed either by the transverse processes of the fourth vertebra alone, or in conjunction with those of the fifth vertebra, or even, as in all probability is sometimes the case, by the partial ossification of its own walls. In some instances also the ductus pneumaticus is either absent altogether, or is represented only by a solid fibrous cord stretching from the air-bladder to the dorsal wall of the œsophagus; nor does the internal ear, or at all events such parts of it as are specially related to the development of the Weberian mechanism, escape retrogressive modification, inasmuch as in some genera the sinus endolymphaticus appears to have undergone complete atrophy.

If it be admitted that the Weberian apparatus and air-bladder function as a register of hydrostatic pressure variations in the *Siluridæ normales*, it becomes almost impossible, in the light of the widespread degeneracy of those structures in the *S. abnormales*, to believe that any such function can be assigned to them in the latter group, or that they are otherwise than rudimentary and functionless organs. Such retrogressive modifications as are involved in the suppression of the ductus pneumaticus, the marked reduction in the size of the air-bladder and its more or less complete encapsulation by bone, seem to me to be conclusive on this point. As to the cause of this degenerate condition of the air-bladder in these forms, there can be but little doubt that it is the assumption of a purely "ground" habit of life, whereby the air-bladder, as a hydrostatic organ, becomes a totally useless organ. From the high degree of specialization which characterizes the air-bladder and

Weberian mechanism in the *Siluridæ* it is by no means surprising that these structures should be liable to rapid degeneration when the necessity for the exercise of their special functions no longer exists, and the numerous and often extraordinary modifications which the air-bladder undergoes in the *S. abnormales* are simply to be regarded as furnishing an additional illustrations of the extreme variability to which all degenerate organs are liable. The wide geographical distribution of the *Siluridæ* and the remarkable variety of habitat and physical conditions under which they live would no doubt constitute a fruitful source of individual variation, and consequently afford adequate scope for the operation of natural selection in perfecting useful organs, and eliminating by atrophy or degeneration all useless or superfluous ones.

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FIFTEENTH SESSION, 1890—1891.

I.—*The Future of Geology.*

By the Rev. G. Deane, D.Sc., B.A., F.G.S.,

President of the Society.

[Presidential Address, October 8th, 1890.]

I purpose, in this my Retiring Address, to make some observations and offer some suggestions as to the Future of Geology. Not, indeed, that I can claim the rôle of a prophet. But there are indications in the tone and manner of recent discussions and research which point clearly to the probable course of geological investigations in the immediate future. Geology has lately become too speculative. For at least a second time in its history we need to pause in order to gather up the records

of the past, and to think seriously about the best method of progress in the future. A century ago, the conflict between the Wernerian and Huttonian theories was at its height. Speculation upon imperfect data ruled all. The practical work of William Smith and other English geologists, and the common sense of Englishmen stayed this mad theorizing; and in the year 1807 the Geological Society of London was established for the express purpose of observing facts and recording observations. The results of this transference of energy from fierce controversy and dispute to patient investigation and labour of detail have been magnificent And now again the old lines of controversy are re-opened. The extraordinary revelations of recent microscopical research, and other improved modern methods, have necessarily re-acted on Physical Geology; and there is once more great danger lest patient labour and accurate induction from proved data should give place to wild theorizing and acrimonious controversy.

The Future of Geology depends primarily on its practical uses. The method of its study, also, including the possible discovery of new methods of research, must be a potent factor in its development. Not less important is the consideration of the problems, whether physical, stratigraphical, or biological, which at present demand solution. And, as in all sciences and in all life, surrounding influences and environment have much to do with growth and progress. On each of these four points I wish to make a few remarks.

1.—The practical use of Geology has received a striking illustration during the past year. Thirty-five years ago Mr. Godwin Austen maintained the probability of Coal Measures beneath the newer strata of the south-east of England; and such Coal Measures have now been found. To the civil engineer, to the miner, to the stone-mason, builder, and architect, to the well-sinker and searcher for water, a knowledge of geology has become essential; and even so humble a person as the modern farmer and gardener might learn something from the coprolite and manure beds of the geologic series, and from

the disposition of rocks and soils. During the past year nineteen committees appointed by the British Association have been concerned with geological subjects. Some of these are speculative and theoretic, but the majority are of practical use. The rate of increase of underground temperature has a direct bearing on mining operations ; the circulation of underground waters is of great importance to the water supply of towns and cities ; the manure gravels of Wexford may revive the husbandry of Ireland ; the flora of the Carboniferous rocks may throw light on the origin of coal and the probabilities of its occurrence in new localities ; and even such apparently theoretic themes as the fossil Phyllopoda of the Palæozoic rocks, the Higher Eocene beds of the Isle of Wight, the erratic blocks sacred to our friends Dr. Crosskey and Mr. Martin, or the collection and identification of meteoric dust, may prove of importance in a practical direction. But not the least valuable are those researches which deal with foreign localities ; and the Atlas range of Morocco, the earthquakes of Japan, and volcanic phenomena of Vesuvius may vie in value with the Bridlington sea beach, and the action of waves and currents on the beds and foreshores of estuaries. So long as British enterprise is forcing its way to the centre of Africa, and exploring the Australian Continent and the larger islands of the East, a fuller knowledge of foreign geology becomes imperative.

But apart from these matters of general interest or world-wide importance, the practical use of geology is exemplified in the researches of individual observers, and of societies like our own. The investigations of Dr. Callaway in the Uriconian rocks of Shropshire ; the discovery by Dr. Lapworth of the Pre-Cambrian rocks at the Lickey and Nuneaton; and the more recent discovery by Mr. Landon of the Lower Bunter Sandstone at Barr Beacon, considerably to the east of the South Staffordshire Coalfield, which had hitherto been thought to be its limit, are all illustrations of what may still be done by patient and zealous work. The crown of future success rests on the brow of toil and thought. Even the things of theory and speculation

ofttimes become exalted into practical service by the growing developments of advancing knowledge, and the varying demands of human progress.

2.—I advance now to the method in which Geology should be studied, and the possible discovery of new means of research. The day has long gone by when geology could be viewed "as a fashionable toy that everyone who has been to school is supposed capable of handling." No one now dares to touch its problems without some knowledge of physics, mathematics, biology, and chemistry. When Dr. Buckland led his tribe of random riders amongst the Oolitic strata of Oxford, or when Sir Roderick Murchison discoursed in sapient language on the rocks of Siluria, geology might have been a "fashionable toy." But not so now. The stern requirements of modern days have made it more accurate, and rendered it more sure. And with its enlargement has come an attention to minute detail, an observance of processes and of the results of processes which in the olden days had no place. To be a geologist now, at all events in the special phases of the science, a man must be either a mathematician, a physicist, a master of biology, or of chemistry. Happy the man who can combine the whole!

Professor A. H. Green, of Oxford, in his recent address as President of the Geological Section of the British Association, at Leeds, discoursed on the value of geology as an educational instrument. He began by admitting candidly that geologists were in danger continually of becoming loose reasoners, because the data from which they reasoned were necessarily scrappy and the geological record imperfect, or the facts were capable of interpretation in more than one way, or the determinations were shrouded in mist and obscurity. Notwithstanding this, he urged that the study of geology would be useful educationally by teaching wariness when the pupil comes to handle the complex problems of morals, politics, and religion. Further:—"There are immense advantages," he continues, "which the science may claim as an educational instrument. In its power of cultivating keenness of eye it is unrivalled, for it demands both

microscopic accuracy and comprehensive vision. Its calls upon the chastened imagination are no less urgent, for imagination alone is competent to devise a scheme which shall link together the mass of isolated observations which field work supplies; and if, as often happens, the fertile brain devises several possible schemes, it is only when the imaginative faculty has been kept in check by logic that the one scheme that best fits each case will be selected for final adoption. But, above all, geology has its home, not in the laboratory or study, but *sub Jove*, beneath the open sky, and its pursuit is inseparably bound up with a love of Nature, and the healthy tone which that love brings alike to body and mind." (*Times* report, September 5th; *Nature*, vol. 42, p. 455.) Professor Green proceeds to argue that geology should be taught in schools for more prosaic reasons; the two chief of which are that geography, and especially physical geography, cannot be taught without constant reference to certain branches of geology, and that there are many points of contact between the history of nations, the distribution and migrations of peoples, and the geological structures of the lands which they have dwelt in or marched over. And he concludes by sundry good illustrations of such school teaching, into which I need not now follow him.

Many may be disposed to think that this able and admirable address of Professor Green is over-strained and over-stated. But it must be remembered that, in order to convince the British public, it is necessary to state things strongly, and to draw things clearly and deeply. I do not myself believe that the average school boy and school girl need to be taught "wariness" in matters of morals, politics, and religion. They are generally wary enough about these matters, as it is; and, what is more, soon get weary of them. And possibly, too much stress is laid upon the relation of geological structure to the history of nations and the migrations of peoples. An enthusiastic geologist might feel inclined to generalise that, inasmuch as the two great battles of the Franco-German war of 1870—Gravelotte and Sedan—were fought upon Jurassic strata,

therefore such strata facilitates military operations, and all the great battles of history were fought upon Jurassic strata.

But, allowing that there may be some over-statement and special pleading, there is a great amount of truth in the address from which I have quoted. Many people go through life with their eyes shut. They do not really see what they think they see ; and what they think they see is not what they ought to see—not what exists and presents itself to them. Whatever, then, trains to accurate observation of facts and phenomena is of value as an educational instrument. The great end of education is to call out and train the powers we possess, whether of mind or body. Whatever, therefore, develops and strengthens a faculty will futher this end ; and that scheme of training which best fosters *all* the powers of mind will be he most satisfactory. The mathematician is apt to think lightlty of the probable and moral reasoning of the philosopher, and the classic sometimes prone to scorn the pretensions of science. For the highest kind of education, a severe, long continued, and isolated application to one special branch of study is requisite ; though there is then great danger that the mind will become warped and one-sided. But, for the initial stages of education, the pursuit of a definite science will tend to supplement and complete the discipline of other studies, and to render the juvenile mind *totus, teres, atque rotundus.*

Professor Green has done good service in urging the claims of geology in this direction. It can never take the place of physics or chemistry ; but it is at least the equal of either biology or physiology for training the mind to accurate and complete observation of facts and phenomena. The word has come from the chair of authority not a day too soon ; and it is to be hoped that ensuing years may witness a great extension of geological teaching in our schools for both boys and girls.

The British Islands form the natural home of geological science. Though limited in area when compared to other countries, they contain in this small space a very great variety of different strata. Indeed, with one or two exceptions of no

very great importance, the whole succession of geological rocks is represented in our land. Though we have not the immense development of one particular formation which occurs in some other regions, we have an immense variety of formations. The surface of our country is a miniature picture of the whole geological series; and England, Wales, Scotland, and Ireland thus become the key to the world.

Accordingly it is not surprising that, in Britain, Geology has been an honoured science, and that its leading votaries have attained a world-wide reputation. Other lands have nobly borne their share in discovery and speculation; but I think we may justly claim for Britain the crown of geological progress. As a science, geology began to emerge in Italy in the 16th and 17th centuries. Pythagoras, Strabo, and others of the ancients, had indeed speculated on cosmogonies; but in Italy, at the time mentioned, the fossils of the sub-Apennine hills led to genuine geological controversy. A century later, Werner in Saxony, and Hutton in Edinburgh, were the great teachers of the science; and at the beginning of the present century the formation of the Geological Society of London based the study of Geology upon a rigid induction of facts, and tended to discountenance crude speculation and hasty theory. Since then the nations of Europe and America have striven in friendly rivalry to further the knowledge of the science they love so well.

I am well aware that in two branches of study, as I shall explain further on, Britain until recently has lagged behind other countries. But this notwithstanding, Britain may well claim to be amongst the foremost in geological research. And we may to-night remember that Birmingham is the centre and apex of England; and that within a comparatively short distance from this room, nearly all the geological formations can be observed and studied. Few places are more favourably situated for extensive and varied research than Birmingham. Within a distance of little more than fifty miles from this spot you may study the representatives of all the systems of rocks

except the upper part of the Oolitic, the Cretaceous, and the Tertiary groups. And what are fifty miles in these days of railways? At slight expense and slight fatigue, a day's fresh air and bracing work full well repay the geologist. And within almost walking distance, abundant materials for geologic study and pleasant recreation may be found. The Clent Hills, the Bromsgrove Lickey and the Severn Valley, the Clee Hills, the rocks of Ludlow, Wenlock, and South Derbyshire, the hills of Dudley and Rowley Regis, of Barr Beacon and Sedgley, of Coalbrookdale and the Wrekin, of Church Stretton, Charnwood Forest, Malvern, and the Cotswolds, are all within easy reach. And these together represent almost the whole of the series of known rocks, whether igneous, sedimentary, or metamorphic.

I have claimed for Great Britain pre-eminence in geological research. I might also claim for Birmingham pre-eminence in one, if not in both, of the branches of geological study which Great Britain has until recently neglected. In the chemical constitution of rocks, and their microscopical structure, the Continental geologists have in years gone by done better work than the English. But we are in recent years striving to wipe away this reproach. Forty years ago Mr. Sorby, of Sheffield, commenced his microscopical researches; and, in Birmingham, Mr. Samuel Allport has been conspicuous for success in this line of investigation. Mr. Teall, of the Geological Survey, Professor Bonney, and many others, have amply redeemed, in this respect, the credit of the country. At the present time Dr. Callaway, Mr. T. H. Waller, and others, are making large use of this method of enquiry.

The chemical relations of geological questions do not even now command in England the attention they deserve; as there is a wide field of research for a qualified chemist in geological problems.

The study of a complex science like Geology, then, includes almost all other sciences. In order to understand the real bearings of the manifold questions that emerge, the geologist

needs to be almost omniscient. He must know something of almost every physical science—chemistry, mineralogy, and crystallography; what is now known by the name of physiography, including meteorology and natural philosophy; biology, physiology, and anatomy are all requisite. Microscopical research is essential. And if the higher parts of mathematics can be added, the observer will be still better equipped.

The past year has seen a new direction of enterprise in regard to the registration of geologic facts. The photographic art has been called into organized recognition. At the meeting of the British Association held at Newcastle-on-Tyne in September, 1889, a committee was formed to arrange for the collection, preservation, and systematic registration of photographs of geological interest in the United Kingdom. In some counties, as in our own, this impetus has taken a rather wider sweep, and aims at a general photographic survey of the county. As is known to many here present, an influential county council has been appointed for this purpose in Warwickshire, with Mr. J. B. Stone as president, and Mr. W. J. Harrison as secretary; and two members of this Society have been delegated to act thereon. Unquestionably many instructive geological sections have been lost to science through want of correct drawing, or photographing, at the time of their exposure; and it is hoped that this committee may aid in recording and retaining such facts. The result of the first year's work has been satisfactory; and the report to the Association at the Leeds meeting shows that 196 geological photographs have been sent in. It is to be hoped that in years to come this method of rendering permanent transient sections may be productive of good.

The possible discovery of new methods of research opens up a wide field of speculation, into which time forbids me to enter at large. Improvements made in modern optical instruments have enabled the geologist to see through a brick wall, or force his way through a prickly problem; and the day may not be far distant when the very centre of the earth will be as clearly seen and understood as its surface now is. One of the

most promising new methods of research is the separation of minerals by means of heavy solutions, and other methods are sure to come. It remains to be seen how far the recent marvellous advances in electrical science may throw light on the problems of rock metamorphism. The transitions from one kind of rock into another are startling and puzzling; and it may possibly be found that terrestrial magnetism and electrical force are potent factors in the results which field geologists have observed and recorded. The genius of discovery is not yet exhausted; and the triumphs of the last fifty years augur an enlargement and expansion in the immediate future which will place the science of geology in the fore-front of human progress.

3.—Passing now to the problems, whether physical, stratigraphical, or biological, which at present demand solution, I may remark that an ex-President of this Society—Mr. William Mathews—in the year 1883, made this topic, with great lucidity and power of reasoning, the theme of his address. Mr. Mathews dealt with four subjects: the doctrine of uniformity, the origin of mountains, the supposed cause of glacial epochs, and the eroding power of ice. As I have myself, in various papers read either before this Society or before the Natural History and Microscopical Society, discussed more or less fully the whole of these subjects, I do not purpose touching them to-night. In fact, the last seven years have brought other questions to the front, on which I may venture to dilate.

Professor Huxley, in his address as President of the Geological Society of London, in the year 1869, traced three phases of geological speculation, viz., the Catastrophism of Murchison and the older geologists, the Uniformitarianism of Hutton, Playfair, and Lyell, and what he styled Evolutionism, which adopts all that is sound and good in the other two, and is destined to swallow them up. He argued that both Catastrophism and Uniformitarianism had kept alive the tradition of precious truths: Catastrophism in insisting upon the existence of a practically unlimited bank of force, on which the theorist

might draw ; and Uniformitarianism in insisting on a practically unlimited bank of time, ready to discount any quantity of hypothetical paper. And he maintained that there is no sort of theoretical antagonism between the two, as it is very conceivable that catastrophes may be part and parcel of uniformity ; and still less is there any necessary antagonism between either of these doctrines and that of Evolution, which embraces all that is sound in both, and applies the same method to the living and not-living world.

I consider the position thus taken up by Professor Huxley to be absolutely impregnable. It is well to bear in mind that geology knows at present no finality. Time-honoured views have been shattered and pulverized by the recently published papers on the Highlands of Scotland, and by both Continental and English geologists on the structure of the Alps. And no one could have attended the London meeting of the Congrès Géologique International without seeing that grave changes in geological conceptions, and in geological interpretations, are impending. The school of Evolution will indubitably swallow up those of Catastrophism and Uniformitarianism.

And if Evolution thus bids fair to dominate physical and stratigraphical geology, its influence can also be traced in the succession of living forms. Professor Huxley himself has done as much as any living man in this direction. In his well-known lecture before the Royal Institution on February 7th, 1868, he conclusively showed that bird fossils are reptilian in their character, and the reptile fossils of the Secondary rocks are bird-like in character. And further, in his lectures at the Royal School of Mines, in 1876, he gave a long and exhaustive series of transitional links, from the Ceratodus of the Trias, which affords a connecting link between Fish and Amphibia through the Reptilian types of the Secondary rocks, to the protohippus, hipparion, anchitherium, palæotherium, and orohippus of the Tertiary rocks, which are links of transition from the modern horse to the rhinoceros, the pigs, and the ruminants.

The recapitulation theory of development, as expounded in the writings of Fritz Müller, von Baer, Balfour, and Haeckel supports the theory of evolution; and has been explained and illustrated at great length and with much power by Professor A. Milnes Marshall in his recent brilliant address to the Biology Section of the British Association at Leeds. He writes:—"The doctrine of descent, or of evolution, teaches us that as individual animals arise, not spontaneously but by direct descent from pre-existing animals, so also is it with species, with families, and with larger groups of animals, and so also has it been for all time; that as animals of succeeding generations are related together, so also are those of successive geologic periods; that all animals living or that have lived are united together by blood relationship of varying nearness or remoteness; and that every animal now in existence has a pedigree stretching back, not merely for ten or a hundred generations, but through all geologic time since the dawn of life on this globe.

"The study of development, in its turn, has revealed to us that each animal bears the mark of its ancestry, and is compelled to discover its parentage in its own development; that the phases through which an animal passes in its progress from the egg to the adult are no accidental freaks, no mere matters of developmental convenience, but represent more or less closely, in more or less modified manner, the successive ancestral stages through which the present condition has been acquired.

"Evolution tells us that each animal has had a pedigree in the past. Embryology reveals to us this ancestry, because every animal in its own development repeats this history, climbs up its own genealogical tree."—(*Nature*, vol. 42, p. 468.)

The theory of Evolution, then, in some one or other of its forms, must be accepted as the basis of the geology of the future. The physical problems which in past years have been examined and discussed by Sir William Thomson, M. Delauney, the Rev. Osmond Fisher, and others, have in recent

years been still further elucidated; conspicuously so by our excellent secretary, Mr. Davison, and Mr. T. Mellard Reade. The stratigraphical problems have called in the aid of thrust planes, reversed faults, dynamo-metamorphism, and catastrophes to alter the dead level of uniformity. And the biological problems are explicable only on some theory of Evolution. In Huxley's words, Evolution is destined to swallow up the other two theories.

Perhaps the most striking development of modern geology is the rise and growth of the Congrès Géologique International; and the questions discussed thereat are, of course, the prominent questions of the present time. Beginning its existence at Paris in 1878, it has since met at Bologna in 1881, Berlin 1885, London 1888, and the next meeting is fixed for Philadelphia in 1891. Its growing importance is indicated by the numbers of foreign members in attendance. These were—Paris 110, Bologna 75, Berlin 92, London 151, and our American cousins next year, as is their wont, will probably "whip creation." As I had the pleasure of attending the London meetings, I read a paper thereon before the Geological Section of this Society, and allude to the subject now only because the topics of the Congress suggest the matters which are under immediate discussion. These were three—the Map of Europe, Nomenclature and Classification, and the Nature and Origin of the Crystalline Schists.

The geological map of Europe is under the care of an influential committee, meeting in Berlin, on which Germany, France, Great Britain, Austria-Hungary, Italy, Russia, and Switzerland are all represented. The scale adopted is 1:1,500,000; that is 1 inch to 23·673 miles; and the map will consist of forty-nine sheets. Some parts of Central Europe are on the eve of publication; and, although the colours are somewhat different from those we are accustomed to in England, it will be a great advantage to have uniformity of colouring for all European countries.

On Nomenclature and Classification, the Congress, having at the previous meetings dealt with the unification of geological terms, gave attention to the classification of the Quaternary and the Cambrian and Silurian strata. It was generally felt that, notwithstanding the insignificant thickness of the Quaternary strata, the advent of man and the existing mammals was sufficient to render this epoch absolutely distinct from the Tertiary. But on the great Cambro-Silurian question a battle royal ensued. As I have treated this fully in my previous paper, I must not take up time to-night upon it. This controversy still rages, both at the Geological Society of London, and in the pages of the "Geological Magazine." Professor Blake is mad on his Monian system; Dr. Hicks is naturally jealous for his Dimetian, Arvonian, and Pebidian of the St. David's promontory; Dr. Callaway is equally sensitive as to his Uriconian rocks of Shropshire; Dr. Lapworth, the inventor of the term Ordovician for the Upper Cambrian of Sedgwick and its equivalent the Lower Silurian of Murchism, may yet have something more to say before the controversy closes; and some of the Continental and American geologists seem to think the whole thing a storm in a teapot, and appear disposed to adhere to the ancient lines.

Closely connected with this controversy as to the base of the sedimentary rocks, comes the discussion concerning the Nature and Origin of the Crystalline Schists, which occupied two morning sittings at the Congress. Here again modern researches tend to subvert the older theories. Dynamic metamorphism, accompanied by recrystallization on freshly induced planes, curves, and surfaces, is now held to explain the most extraordinary transitions from one kind of rock into another. Both chemical analysis, aided by new methods, and microscopical investigation, with improved instruments, establish this conclusion; and it would seem likely that the immediate future would realise the reasoning of Hutton and Playfair that the Sedimentary Rocks give no indication of a beginning and no prospect of an end. Certain it is that the indications of bedding and sedimentary

origin are encroaching fast upon what was only a short time ago considered part of the primeval crust of the earth. Some cases of supposed bedding, as for example in the Malvern crystalline axis and in some districts around the Wrekin, have been shown to be connected with igneous rock probably re-arranged under great pressure. But as to the origin of the crystalline schists, Professors Heim, of Zurich, Lehmann, of Kiel, Drs. Lapworth and Callaway among ourselves, Dr. Hicks, and many other most able investigators are firmly convinced that mechanical pressures and deformations are in reality the cause of the most sensational changes from both sedimentary and igneous rocks into crystalline schists. No doubt the old conflict will come on again, and it will be many years before these views will be universally accepted. But that they are destined to dominate the immediate future is as clear to me as the shining of the sun on a bright day at noon. The molecular changes induced by vast pressure, and its accompanying natural forces are quite sufficient to change the structure and nature of crystals and rocks. Investigations in the field and in the laboratory will soon set these points at rest, though for some years to come the conflict of opinion may be strong and fierce. The chief difficulty at present is the apparent elimination of alumina and magnesia; but I have little doubt our chemists of the future will solve this problem, and their researches will throw light upon the nature of these widely extended though little known substances. When practical geologists speak of the crystalline schists and associated strata as "a jumble of rocks," it is time some one arose to reduce the "jumble" to order; and there is every reason to believe that chemical and microscopical research will speedily bring him.

4.—The last element in the future of geology which I propose to speak of may be expressed as the external influences which bear upon the development of the science; so to speak, its environment.

Looking around at present upon geological activities in Britain, we find the Geological Society of London, the

organization represented by the Geological Survey, a number of Societies scattered through the kingdom which are devoted, either solely or partially, to the furtherance of geological research, and a large number of earnest individual workers in almost every nook and corner of the land. To these must be added the Royal Society, the Professors of the Science Colleges, with the great influence they spread, the British Association for the Advancement of Science, and the influence of the Universities in geological directions. With this vast army of workers geology *must* advance. But, as in the past so in the future, it will be an advance amongst difficulties, and in the face of opposition and obstruction.

The Spirit of the Age has a mighty power on all things; and it might be thought, at first sight, that the Spirit of the Age would urge the science of Geology forwards at almost headlong speed. But I am not at all so sure of this: it may urge general scientific enquiry forwards, but the popular directions do not run on geological lines so much as on some others. To put the matter in a nutshell, geology does not pay; and it must be made to pay, before competent and trained men will be able to give time and toil to its pursuit. Very few competent persons can afford to give up their leisure, and *also* their money, from a mere love of the science.

The last twenty years have witnessed a great expansion in scientific matters. Science Colleges have been established in many of the great centres of population, and to most of these a Professor of Geology is attached; in the Board Schools of most large towns and cities, Science is taught, and with it Geology to a greater or less extent; more often less, and sometimes meagre. Private schools and organisations, likewise, sometimes favour, generally tolerate, the study. But still we are not happy. These things are not as they should be. Geology may reasonably claim a prominence it has not yet received.

In the days of Sedgwick, Buckland, Chalmers, Hugh Miller, Lyell, and Murchison the leading geologists might be counted

on your fingers; now they may be counted by scores, and it may confidently be expected that, notwithstanding pecuniary disability and in defiance of difficulties, the numbers will still increase. But at present the superior advantages of other lines of scientific thought and effort draw many away from the geologic path. Those who remain are attracted more by love of truth than hope of pay. If the amount of money sunk and lost through want of correct and accurate geological knowledge could be fully estimated, its total would be astounding. Some well laid schemes, under good geological direction, doubtless have failed; but such are very few. The successes have greatly exceeded the failures. Here, close to Birmingham, the Sandwell Park Colliery may be pointed to; and the "Search for Coal" Committee in the South-East of England will, in all probability, render a good account of their labours. With such examples before us the public zeal for geology ought to be greatly stimulated.

In estimating the Spirit of the Age with regard to Geology, one element ought to be noticed, which I rather shrink from introducing here—I mean the past theological hostility to the science. I will, however, deal with it generally, without introducing controversial matters. This hostility is scotched, but not killed, as it ought to be by this time. In centres of intelligence it has now but little or no power, but it lingers in the dark places of the land. A perverted theological bias has never yet succeeded in preventing the ultimate advance of a correct and accurate science; but it may hinder and obstruct. Lukewarm friends are little better than open enemies; and unconscious influence, from the cause referred to, may, and probably does, hold back many from hearty and earnest support of geological work. What is wanted for genuine and full growth and progress is the earnest and sympathetic aid of all classes and conditions. If this be withheld, the growth will languish, and the progress will lag. Compared with the past, indeed, we have reason for immense thankfulness; but the evil still lurks, and has yet to be faced and finally destroyed.

And now, to apply this Spirit of the Age to concrete existence, and attempt to read the future in the light of the present, that future will depend upon at least two main supports, viz. :—1st. The union and the rivalry of effort; and, 2nd. The devotion of either public or private money to geological objects, and to the determination of crucial points.

1st.—The union of effort is represented by the many Societies already referred to scattered over the face of the country; by the central bonds of the Geological Society of London and Section C of the British Association; and by the newly developing Congrès Géologique International, which promises good service in the common cause. Some of these Societies, Field Clubs especially, are more of the pic-nic and social character than is likely to conduce to effective progress of scientific research; and even the meetings of the British Association are open to some criticism on this score. In our Midland district, in 1876, a new departure was started in the Midland Union of Natural History Societies; and this, after some vicissitudes, is still living a vigorous life; held a highly successful annual meeting at Oxford last year; and this year at Leicester a most pleasant meeting has been held, accompanied by two well-planned excursions, one botanical and the other geological, through Charnwood Forest.

For one, I am not disposed to value lightly the influence of even mere social gatherings connected nominally with Science. They tend to give a tone both to the neighbourhood where they are held, and to those who attend them; and also put people on the alert for possible discoveries. The experts in geology are few, but the watchers and labourers are many; and these last, scattered as they are throughout the country, may hear of or find out facts and points of interest which the experts may subsequently be called upon to examine and explain. Many an interesting geological fact, or even crucial section, has been lost simply because no one who understood the matter was at hand to decipher and preserve it, or report it to those who could do so. By all means let us increase our

army of observers; what they hear of and discover experts can explain. A striking illustration of this kind of labour has recently occurred in this district. Mr. Sherwood, of Sutton Coldfield, found a freshly opened section near Barr Beacon, which exposed rocks that were new to him. He reported the fact to Mr. Landon, of Saltley College, who discovered an eroded surface of the Lower Bunter Sandstone, in a locality where it had previously been believed to be absent. Mr. Landon has since discovered quarzite implements in a river gravel at Saltley. I have had, in my own experience, similar illustrations, one of which is worth recording. When residing in the valley of the Ouse, in Bedfordshire, during an occasional absence from home, a well was sunk near my house. The workmen came upon the lower jaw of a hippopotamus, and of course proceeded to demolish it with their pickaxes. A friend of mine happened to pass, and he succeeded in saving for me some fragments of teeth and jaw. When I returned, the bulk of the remains had been used as stuffing to the back of the well. But my friend had saved sufficient to prove the existence of *Hippopotamus major* in that locality.

This union of effort necessarily involves some amount of friendly rivalry. It seems to be a law of humanity that two vigorous persons, jogging side by side along the same road, stimulate each other to increased pace, And so, in each society the blending of effort is a stimulus to each individual worker. In a union of societies, the same power should be felt; each will vie with the others, not simply for pre-eminence of course, but for progress. And the result comes unconsciously in the advancement of the object they have in common. Geologists in the nations of Europe and America, organized in various societies, and surrounded by different influences, have one common object, and mutually stimulate each other towards the attainment of full and complete geological knowledge. It may be, sometimes, that this rivalry will lead to strenuous conflict; but conflict of opinion and thought, so long as personal rancour and strife are excluded, will always lead onwards in the path of truth.

2.—In approaching the matter of money—whether public or private—devoted to geological objects, I touch a subject of some magnitude, complexity, and difficulty. When the British purse is appealed to, buttons are often in requisition, not in lieu of coins, but to close the exit of coins. But it is perfectly clear that geological investigation is expensive, and the pecuniary resources of most competent geological observers are limited. Geologists have to rely, for the most part, upon natural sections and exposures, or upon those artificial sections and borings which commercial enterprise opens up. A judicious expenditure of money to make artificial sections and borings, in order to determine crucial points, would often be amply repaid.

In the allocation of public money to geological objects, we have conspicuously before us the Geological Survey of the kingdom, the maintenance of the Royal School of Mines, and the Natural History Museum at Kensington. Then, during the last year, a grant in aid of Provincial Science Colleges has, after much agitation, been wrung from the Treasury; though how much of this will find its way to geological objects is very problematic. Perhaps the most significant "sign of the times" in this direction is the report of the Committee, appointed by the Commissioners of the 1851 Exhibition, as to the establishment of Science Scholarships in Provincial and Colonial Universities and Colleges. The second item of the Committee's recommendation runs thus:—"That the scholarships be limited to those branches of science (such as physics, mechanics, and chemistry) the extension of which is specially important for our national industries." (*Nature*, vol. 42, p. 431, August 28th, 1890.) Of course this is a case of complete powerlessness on the part of geologists. The Commissioners are acting within their rights, and after due deliberation. But, with all deference to the illustrious men of science who have drawn up this report, I humbly think that geology ought not to be excluded from the subjects that are specially important for our national industries.

Various scientific societies allocate money in aid of geological research. The grants from the Royal Society and the British Association have been of great service, not only in rousing activity, but also in rewarding, or rather recompensing, worthy work. The Geological Society of London has at its disposal several most honourable awards for well spent labour. The medals come first—the Wollaston, Murchison, Bigsby, and Lyell medals being the highest geological honours of the country. But the surpluses of these funds, as also the "Barlow-Jameson Fund," are allocated from time to time to repay in some measure the expenses of those who have rendered distinguished service and wrought good work. With us in the Midlands the Darwin Medal of the Midland Union and the grants made from our "Endowment of Research Fund" are a humble reflex of such awards. There is ample scope for extension and enlargement in awards of this kind.

But perhaps the development of future British Geology may come from other sources. The enterprise of the South-Eastern Railway directors has opened up the possibility of coalfields where thirty-five years ago geologists said they might exist; and public companies can do much to aid further research. Private enterprise also may do much. This old land of ours is not yet used up, and one need not despair of discovering still, in its soil and rocks, fresh elements of permanence and power.

I have thus endeavoured to trace what seem to me to be the possibilities of Future Geology. There may be regions yet unsearched, which will yield up their treasures to the diligent. The current controversies on theoretical points afford scope for the acutest intellect to unravel and explain. New methods of research give promise of coming discoveries. The Spirit of the Age and the surroundings of the science are favourable on the whole to progress. If money be forthcoming to meet needful expenses, and observers are careful and accurate, the past triumphs of geology will appear small compared to the triumphs that are yet to come.

II.—*A Graphical Method of Explaining the Diffraction Bands at the Edge of a Shadow.*

By J. H. Poynting, F.R.S.,

Professor of Physics, Mason College, Birmingham.

[Read before the Society, Nov. 5th, 1890.]

The case here treated is that of the shadow of a screen with a straight edge, interposed in the course of waves proceeding from a source so distant that they may be regarded as plane. A diagrammatic method of representing the effect due to the successive "half-period" elements is adopted, and from the diagram is obtained a general explanation both of the bands without the geometrical shadow and of the rapid decrease of illumination within it.

Let the plane of the paper (Fig. 1) represent a wave front. To find the illumination at any point P in the normal to the wave front through O, we adopt the usual plan of breaking up the wave front by a series of concentric circles round O as centre, and such that their distances from P increase successively by $\frac{\lambda}{2}$. That is, $\frac{\lambda}{2} = PA - PO = PB - PA = PC - PB =$ &c. The innermost circle and the circular bands thus formed are termed half-period elements. In Fig. 1, ten of these elements are represented, drawn their true size, for a wave length of sodium light ($59/10^5$ mm.), when P is 200 metres in advance of the wave front. If OP is 1 metre, OA will contain 200 elements, and the first will have a radius about $OA/14$.

Fig. 1.

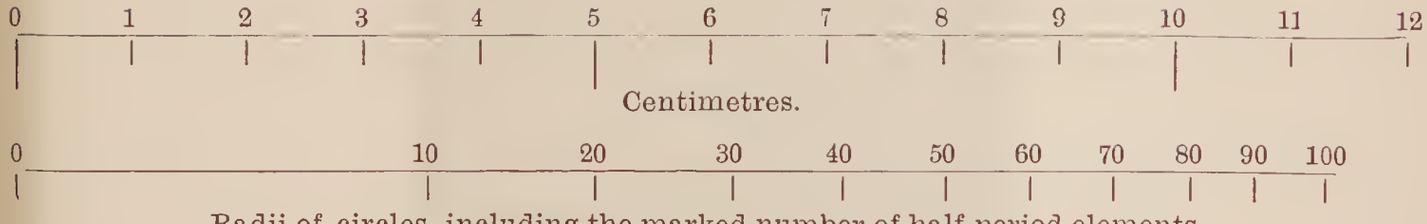

Fig. 1.—Wave front divided into circular bands, increasing successively by a half wave length of sodium light (·00059 mm.) in distance from a point 200 metres from the plane of the figure. Underneath are the radii for every tenth band up to the hundredth. Actual size.

Taking each element to be the source of an independent disturbance transmitted to P, since the mean distance of any element differs by $\frac{\lambda}{2}$ from the next preceding or succeeding, the series of disturbances arriving at P at any instant are alternately in opposite phases and tend to neutralise each other. Assuming that the amplitude of the disturbance sent by any small area is proportional to area / distance, that sent by a

circular band of radius ρ, breadth $d\rho$, distant r from P, may be represented by $2\pi\,\rho\,d\rho/r$ (omitting the constant factor).

But $\rho^2=r^2-d^2$ where $OP=d$. Hence (or by a very easy geometrical proof) $\rho\,d\rho=r\,dr$, and the disturbance is $2\pi\,d\,\rho$. Now $d\rho=\frac{1}{2}\lambda$ for each element, so that each sends the same disturbance $\pi\lambda$.

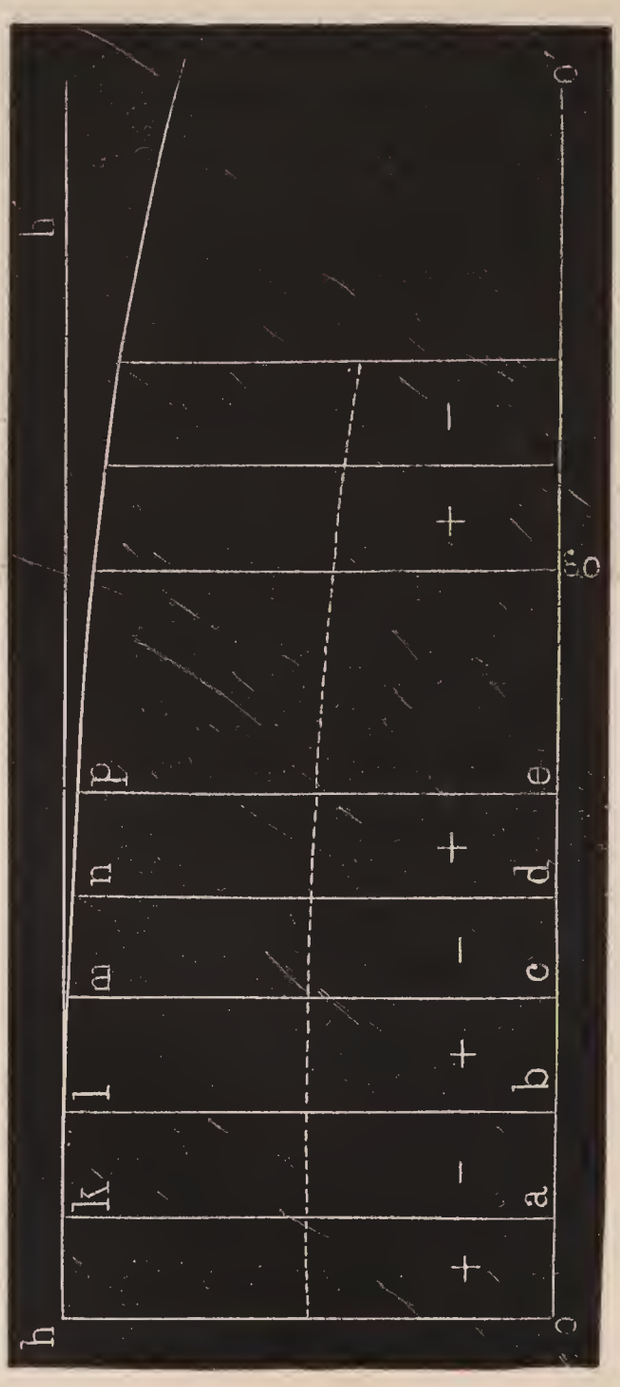

Fig. 2.

Fig. 2.—Areas representing the disturbance at P, due to successive half-period elements. The curve *h k l m* shows the diminution due to inclination. The dotted curve has half the height.

But this takes no account of the inclination of the line of propagation to the normal to the wave front, whereas we must suppose that the disturbance propagated decreases as this line gets further from the normal.

To represent the effect due to successive elements, take along a line $o\,o'$ (Fig. 2) equal distances, $o\,a = ab = bc = \ldots = \frac{\lambda}{2}$ draw a line, $h\,h'$, parallel to $o\,o'$ and distant 2π from it, and draw perpendiculars through $o\,a\,b\ldots$ to meet this line. A series of rectangles, each of area $\pi\lambda$, is thus formed, and they may be taken as representing the effects of the elementary bands, with breadths $O\,A$, $A\,B$, $B\,C$, in Fig. 1, on the supposition of no diminution from inclination. Now, draw a curve, $h\,k\,l\,m\ldots$ such that its height from any point in $o\,o'$ represents the effect due to a half-period element about the corresponding point in Fig. 1. This curve will slowly descend and ultimately approach indefinitely near the base line $o\,o'$. It cuts off the tops of the rectangles, and the areas beneath it, gradually diminishing from $\pi\lambda$ to 0, represent the disturbances sent by the successive elements. As these are alternately in opposite phases, tending to neutralise each other, they are

Fig. 3.

Fig. 3.—The area of the positive elements left when the negative elements have been folded over.

marked alternately + and −. Thus, the second nearly neutralises the first, the fourth nearly neutralises the second, and so on, each pair, however, leaving a small balance over of the same sign as the first element. To find the sum of all these small remnants, imagine the figure cut out in paper: white in front, black at the back, and with folds made alternately in opposite directions, like those of a fan, along the lines $a\,k$, $b\,l$, $c\,n$, &c. Now, folding the whole figure down on to the first element, evidently the white area left will be the sum of the remnants. In Fig. 3 the white area is represented, and it is seen to consist of a very great number of triangles, each of height $o\,a$, and with total base $o\,h$; so that the total area is half that of the rectangle $a\,h$, and the total disturbance is half that which would be produced by the first element alone.

We may at once see the effect at P of the interposition of a small round opaque disc, with centre at o. We must draw half-period circles, *starting now from the edge of the disc*, and Fig. 2 will still represent the series of separate disturbances if we begin at a point g, some way from o. The total effect will still be as in Fig. 3, half the first area at g, which is nearly equal to the area $o\,k$, since the curve $h\,k\,l$ only descends very slowly. The effect at P is nearly the same as if the opaque disc were removed. We might also easily show that there are successive bright and dark regions along the axis of a small circular hole in an opaque screen.

Let us now examine the effect of a screen with a straight edge stopping out any part of the wave. We shall suppose the screen to extend indefinitely on the left and consider the disturbance at P, as the edge occupies various positions with regard to O.

First, let the screen pass through O (Fig. 1) so that P is at the edge of the geometrical shadow. In this position the screen merely cuts off half of each element, and the areas in Fig. 2 must be reduced to half their height as represented by the dotted line. The result of folding over is to leave half of

the reduced first element, so that the total disturbance is half as great as that due to the uninterrupted wave, and the intensity (varying as the square of the disturbance) is one quarter.

Next let the screen be drawn to the right so that P advances without the geometrical shadow. Every half-period

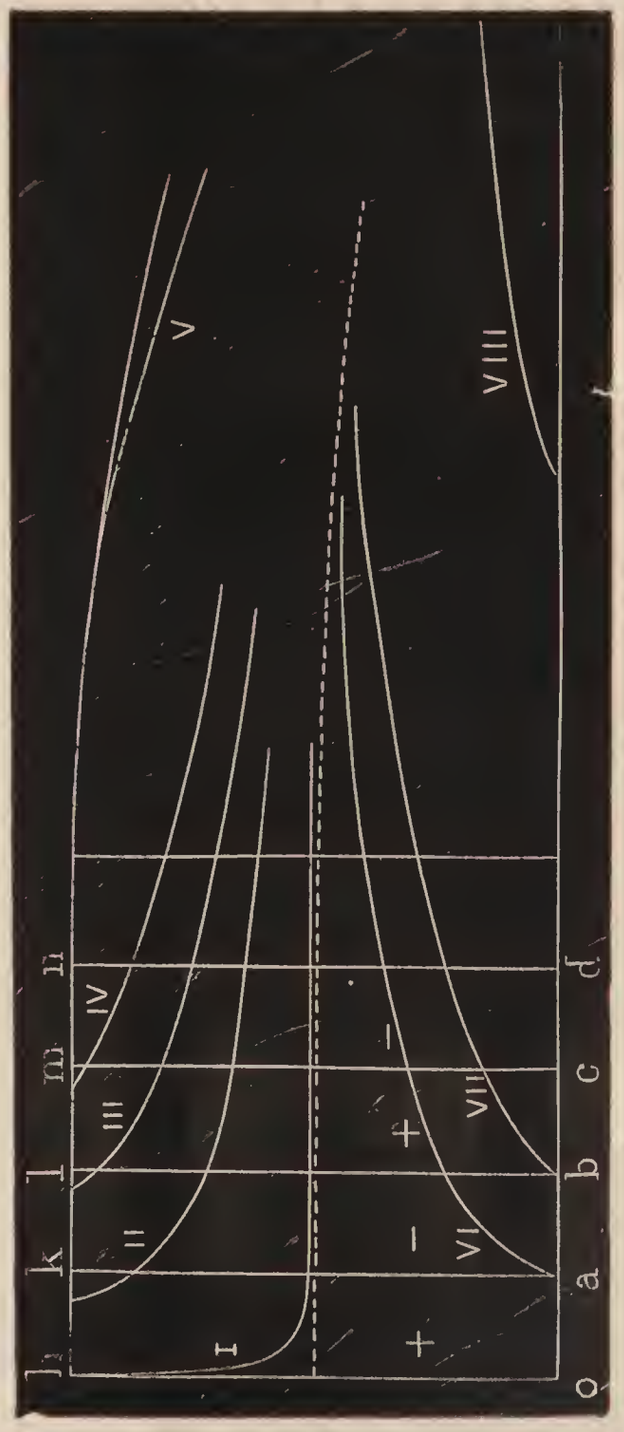

Fig. 4.

Fig. 4.—Showing the amounts cut off the successive half-period elements by the screen. In I.-V. the screen is to the right of O (Fig. 1). In VI.-VIII. it is to the left of O.

element in Fig. 1 has now more than half its area left, though the areas of the elements distant from *O* decrease towards half

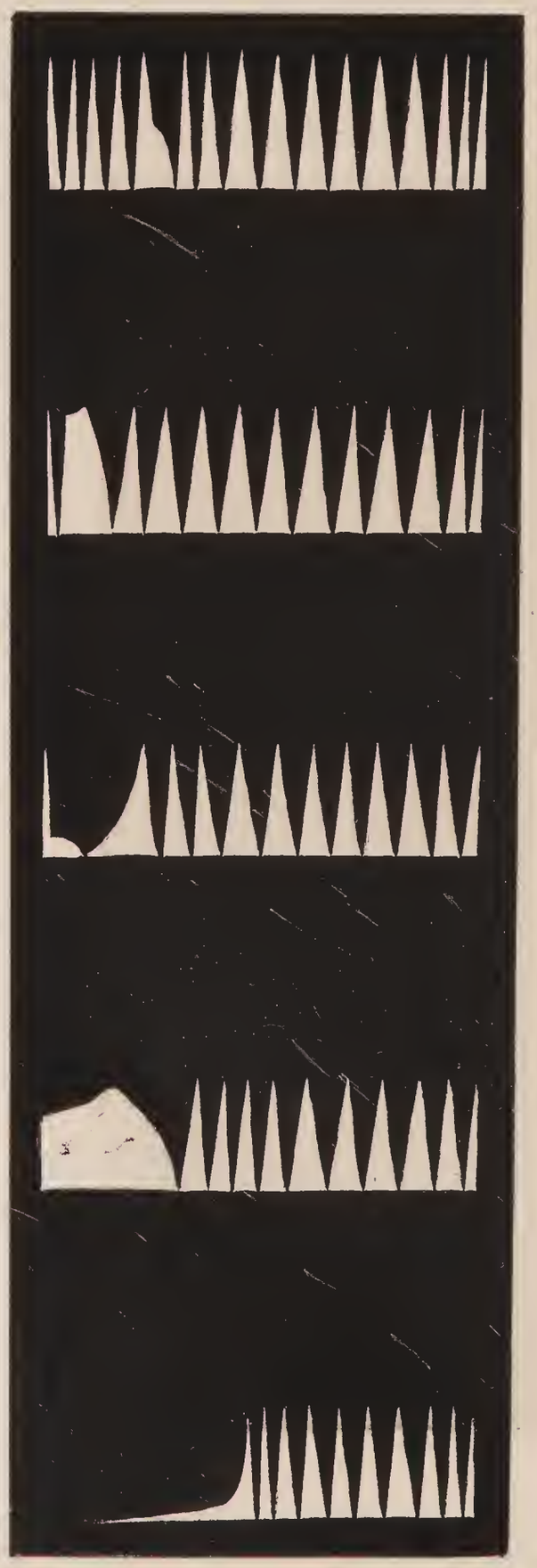

FIG. 5. Just without the geometrical shadow.

FIG. 6. First maximum.

FIG. 7. First minimum.

FIG. 8. Second maximum.

FIG. 9. Distant maximum.

their full value as a limit. Further, the rate of increase in the amount cut off from each successive element by the screen is more rapid as the edge is nearer O. This may easily be seen by laying an edge on Fig 1 in different positions. Thus, if it passes through A, very soon a large fraction of half of each element is cut off; while, if it passes through E, the approach to the half element is much more gradual.

The curves I—V (Fig 4) represent the reduction in effect —the amount cut off each element—when the screen is placed in Fig. 1, just to the right of O, just to the left of A, of B, of C, and finally some way from O. Figs. 5—9 represent the result of folding over in each case, as in Fig. 3.

It is evident that Fig. 5 gives little more than the half effect at the edge of the geometrical shadow. Fig. 6 gives a good deal more than Fig. 3, *i.e.*, more than the illumination when the screen is taken away. Fig. 7 gives considerably less, Fig. 8 somewhat more. That is to say, there are a series of maxima and minima, and exact calculation shows that these are opposite points a little to the left of A B C respectively. Fig. 9 shows the effect when the screen is some distance from O; and now there is only a slight irregularity in the fifth triangle, so that the area is only slightly different from that in Fig. 3. The maxima and minima therefore rapidly die away, and soon the illumination is sensibly equal to that existing when the screen is removed.

If the screen is now put with its edge to the left of O, P is within the geometrical shadow. There will now be a circular area round O, and touching the edge, which will be entirely cut off, and starting from the circumference of this, successive half period elements will have areas which increase, more rapidly the nearer the screen is to O, up to half their full area as limit. This is illustrated by the curves *VI*, *VII*, *VIII*, Fig. 4. To find the effect for any position of the screen we may proceed thus. Any curve representing the effect of successive elements rises nearer and nearer to the dotted half-height curve, but this latter is itself descending, so that ultimately the former curve must

also descend. Take two successive elements about the point of maximum height, and for ease of interpretation it is better to have an odd number of elements to the left of the two selected. Fold down on to these two from each side, and the white areas remaining show how much of each is unneutralised—on the left by the elements up to that point, on the right by the elements beyond it.

Fig. 10.

A short distance within the geometrical shadow.

Fig. 11.

Further within.

Fig. 12.

Some distance within.

Figs. 10, 11, 12 show the nature of the results for curves *VI*, *VII*, *VIII*.

Now the amounts left over in the two elements are in opposite phases, and therefore tend to neutralise each other. But in Fig. 10 the neutralisation is not complete owing to the defect in the left hand through the curvature of the sides of the triangles. In Fig. 11 there is still a defect on the left for the same reason, but much smaller in quantity, and the two areas are more nearly equal. In Fig. 12 they are sensibly the same, and the neutralisation is practically complete. In other words, no light reaches the point.

Evidently, then, while without the geometrical shadow there are a series of bright and dark bands rapidly decreasing in their difference from each other, within the geometrical shadow the illumination always decreases, and so rapidly that a very short distance within there is nearly complete darkness.

Note on the Equation to the Curves in Fig. 4.

The equation is easily shown to be

$$y = \frac{1}{\pi} \sin^{-1} \sqrt{\frac{x}{x+h}}$$

where y is the fraction of the element cut off or left, as the case may be. h is the distance of the starting point from o or h, and x the abcissa, measured from the starting point. If $h = \frac{\lambda}{2}$, *i.e.*, if the curve starts from a or k, we get the following values:

$x =$	$\frac{\lambda}{2}$	λ	$\frac{3\lambda}{2}$	2λ	$\frac{5\lambda}{2}$	∞.
$y =$	·25	·30	·33	·35	·37	·5

If h is increased, x must be increased in the same ratio, to give the same value of y. Hence the gentleness of slope is proportional to h.

III.—*Investigations in Artificial Teratogeny.*

By Bertram C. A. Windle, M.A., M.D.

Professor of Anatomy in the Queen's College, Birmingham.

[Read before the Society, November 5th, 1890.]

With the assistance of grants from the Endowment of Research Fund of this Society, I have for some year or more been carrying on a series of observations on the effect upon the development of eggs of various artificial conditions. Although these observations are far from being complete, I think it advisable to place on record in the Proceedings of the Society the results which I have already obtained, with certain reflections which they suggest, and the lines for further research which they open up. It may, in the first place, be advisable to briefly describe the apparatus which I have employed. During the first part of my work I employed as an incubator the form ordinarily used for bacteria cultivations, with the addition of ventilators and a water tray. The heat was regulated by the thermostat invented by Dr. Nichol, of the Mason College in

this city, to whom my hearty thanks are due, not only for the instrument, but also for the trouble which he took in assisting me to overcome the initial difficulties connected with temperature regulation. At his suggestion, I added between the thermostat and the gas supply a second regulator, of the kind used in the street gas lamps, and thus was able sufficiently to neutralise the constant alterations in pressure which must naturally occur in a large building with many gas supplies coming off from a common main. The regulator in question is a little troublesome to set at first, but once set it works with wonderful accuracy. For various reasons I have in my later observations ceased to use this incubator, and have instead employed one of Hearson's Champion incubators, of twenty-five-egg size. I am using it at the present moment, and I may say that it has given me complete satisfaction. It is easy to set the regulator, after one has once mastered its manner of working, and when set it works most satisfactorily. I use gas and not oil, and find it unnecessary to employ the intermediate pressure regulator which I used in my earlier experiments.

In the electrical experiments hereafter to be detailed, I used a battery which was devised for me by my colleague, Professor Poynting, which he calls a sawdust Daniell. He was good enough to ascertain for me that such a battery was capable of transmitting a current through an egg, which could be distinctly recognised by its effect upon a Thomson galvanometer of low resistance.

I shall now describe the results of the various series of experiments which I have made.

1.—Exclusion of part of air supply by varnish.

As I shall have occasion in a further and more complete paper to describe in greater detail, this procedure has been carried out by many previous observers. In my experiments a greater or smaller part of the egg was covered over with a thick layer of strong varnish, the uncovered part being placed at various parts of the shell. As far as my observations have gone, it appears to me that it does not much matter what part of

the surface is left uncovered, with the exception of the pointed end which seems to be the least favourable to development. In the few cases in which this has been the only part left uncovered there has been a very early cessation of development or no development at all.

With this exception, the effect upon development seems to be much the same wherever the air is admitted, and whether it is admitted by one or by several uncovered areas. An oval patch 2·0 by 1·5 cm. of uncovered shell on that part which is uppermost serves the purpose well, and is the site which I have adopted for the series of experiments upon which I am now engaged. I have so far dealt in this way with thirty-seven eggs, of which fourteen were unimpregnated or showed no signs whatever of development. The following conditions were observed amongst the remainder :—(1) The commonest condition was that where a small misshapen embryo, sometimes merely a heaping up of tissue, occupied the centre of an ill-defined, irregular area vasculosa. In some cases an area of this kind existed where no embryo was to be made out. I am now engaged in tracing out the history of this condition, and so far am inclined to think that an embryo, or some traces of the existence of one, will be found in these cases. At least if the egg be opened within the first forty or forty-eight hours of incubation there will generally be found, if development has commenced, that there is present a small, often irregular shaped, area pellucida, sometimes with indications of the formation of medullary folds, or other traces of further development, but with no signs of vascularity. At a later period the area vasculosa is found with the ill-developed embryo in the centre. As regards the embryo, I can at present only say that in many cases it is represented by a distinct and well-marked heaping up of tissue in the centre of the area vasculosa. I have as number of these amorphous embryos in process of examination, and defer any further remarks upon them until my observation, are complete. (2) In other cases, there is a distinct embryo, which was either well formed, but much smaller than it should

be in relation to the length of incubation, or which presented certain signs of ill-development. The following are examples. (*a*) A batch of eggs, examined after five days' incubation (temp. 100·5 F.). had amongst them two of interest in this connection. The first presented no signs of area vasculosa, but was itself engorged with blood so as to look like a vivid red, or rather purple, mass when the egg was opened. This embryo and the next are now being examined microscopically, and I hope in a further paper to report more fully upon their condition. The next egg opened, from which the air had been excluded in part, in a similar manner to the first, beside which it lay in the incubator, presented a totally different appearance. Instead of being engorged with blood when first opened, it appeared to be quite white. There was no area vasculosa, and the heart could be seen pulsating upon what appeared to be a perfectly colourless fluid. Placed in a watch glass of warm salt solution, under a low power, it became evident that the fluid circulating did contain a few red corpuscles, but very few, not sufficient to give any appreciable tinge to the blood when looked at only with the naked eye. The cerebral vesicles of this embryo were imperfectly developed, and there was no trace of an eye, whilst the whole body was doubled up in an irregular and unusual manner. (*b*) A second batch, incubated at the same temperature, and also examined on the fifth day, gave the following results:—(1) Had had a small and scanty area vasculosa, but was itself over vascular, the heart was large and pulsating, the head and neck were lying on the left side, but the remainder of the body was still ventrally placed. (2) A few streaks of blood here represented the area vasculosa, in the midst of which lay a very small embryo, whose heart was seen to be pulsating upon a colourless fluid, in which, under the microscope, it was impossible to detect any red corpuscles, these being apparently confined to the area alone. (3) A small area pellucida without further trace of development, save for a few flecks of blood around, rendered visible under the microscope. (4) A small embryo with an area vasculosa. This

embryo, though small, was normal in appearance, but lay on its right side. The remainder of the eggs in this batch were examined on the seventh day, with the following results :—(5) An area vasculosa represented by a large ring of flecks of blood, with no distinct vessels and no trace of embryo, so far as could be seen by the naked eye. (6) Apparently unfertilised. (7) Ditto. (8) Similar to 5, save that there were a few flecks of blood visible in the centre of the boundary ring. No embryo was to be seen. (9) A very wide ring represents area vasculosa, but no vessels can be detected passing from it to the small, white, dead embryo in the centre. The latter is about 0·5cm. in length, has a few flecks of blood around it, and apparently possessed only a single cerebral vesicle, but was in a semi-macerated state, which rendered accurate observations impossible. (10) Undeveloped. (11) Contains a small, ill-developed embryo. (12) Undeveloped. I have detailed, at length, the condition of the whole of this batch, because it gives a good idea of the kind of results which one may expect to get by this method of procedure. I need not describe the condition of the other batches examined, since to do so would entail unnecessary repetition. I shall recur to certain points worthy of note in the concluding part of this paper.

2.—Incubation under electrical current.

This procedure has been much less followed by investigators than that which formed the subject of the previous section. I shall therefore mention such references on the subject as I have been able to collect. Dareste, in his book, "Recherches sur la Production Artificielle des Monstruosités," (1) states (p. 54) that "M.M. Prévost et Dumas (2) puis tout récemment M. Lombardini (3) ont obtenu des monstres à l'aide de courants électriques produits par la pile ou par les appareils d'induction." The work of the latter author I have not been able to consult, and the information given in the paper by the first-named two authors is more than scanty, since they simply say, "Nous nous proposions de poursuivre ces recherches, et de donner la description des monstres que l'on produit à

volonté par des variations de température, des altérations de l'atmosphère qui entoure les œufs, et des influences galvaniques, mais il ne nous reste sur ces points que des dessins et des notes incomplètes." In the experiments which I have carried out upon this subject, the shell of the egg was first perforated at its two ends by exceedingly small holes, into which were introduced the pieces of platinum wire used for making the connections between contiguous eggs and the wires from the batteries. The wire in each case penetrated for about one quarter of an inch into the interior of the egg. The aperture being very small was almost completely filled up by the wire; but, in order to complete the closure, I used, in the first part of my experiments, a drop of sealing wax. In the latter part of the series, however, in case the heat of the wax should prove a disturbing factor in the observation, I substituted Canada Balsam dissolved in chloroform, which answers the purpose very well. The wires were connected with the battery, which has already been described.

I shall now briefly describe the results of the experiments which I have made in this direction.

(I.)—Temp. 100·5 F. Twelve eggs were put in, of which eight were connected with the wires whilst four were not, but were placed in the incubator for control purposes, and without having suffered any interference. Of the control eggs, when the batch was examined on the eleventh day, three were found to have developed in a perfectly normal manner. The fourth presented an extended but exceedingly scanty area vasculosa, in the centre of which was what appeared to be a small mass of embryonic tissue without definite shape, in the interior of which was a vessel which I distinctly saw pulsating. I regret that, owing to some accident, this specimen was not reserved for further examination, so that I can speak with no positiveness as to the nature of the central mass; but so far as one could judge from looking at it as it lay in the shell it was absolutely without definite shape. Its size was about that of one half of a small dried pea, or even less. I cannot explain why this egg should

have developed in this way, save that eggs will, under any circumstances at times, develop improperly; and it is only by the fact that so great a proportion do so under artificial conditions that we are able to learn anything about the latter. Beyond this, it may, however, be mentioned that this particular egg had an exceptionally thin shell, and that no water tray was supplied to this batch, so that perhaps these circumstances may have been in some measure responsible for the condition.

I will describe the remaining eggs in order from the positive to the negative end. (1) Unfertilised. (2) An irregular and scanty area vasculosa, containing in its centre a small dead embryo, which was partly macerated. (3) This chick was of full size, but had no eye or appearance of eye on the right side of the head, which was much smaller than the left. (4) Normally developed. (5, 6, 7) Undeveloped. (8) Fully developed as to size, but the abdomen was open, and the allantois, which was small, was hanging out of it.

(II.)—Eleven eggs were put in the incubator, of which eight were connected with the wires, and three were left as control specimens. Of the latter, two were normally developed, the third unimpregnated. Of the former, six showed no signs of development, and were probably unimpregnated, one was normally developed, and one presented an irregular area vasculosa, without further obvious signs of development; this was the egg nearest to the negative pole. I ought to have mentioned that these eggs were examined on the seventh day.

(III.)—Temp. 102·5 F. Twelve eggs (which had, however, been in my possession for a week for unavoidable reasons) were placed in the incubator, eight being connected with the wires, and four being unconnected. Of the control specimens, one was normally developed, two showed no signs of development, and one had a large irregular ring-like area vasculosa, with a few dots of blood and vessels in its interior, but with no further obvious development. Commencing at the positive pole—(1) A large irregular area vasculosa, with numerous spots and flecks of blood contained within its circumference. In the

centre was a small amorphous mass representing a blighted embryo, which was adherent to the shell, so that the contents could be poured out, leaving it behind. I ought to mention here, for fear of being misunderstood, that what I have throughout these notes been alluding to as an area vasculosa is totally unworthy of the former of those names, since it would be much more correctly described as a circulus vasculosus, for it consists of an area non-vascular, or only vascular in the sense of being marked with isolated dots, flecks or streaks of blood, the circumscribing ring consisting of either a row of more or less connected dots, or of streaks forming a seldom continuous ring. (2) Similar to 1. (3, 4, 5) Undeveloped. (6) Was cracked and partly dried up, but presented appearances very similar to 1. (7) Similar to 1. (8) Had a large ring of blood spots the whole size of the upper surface of the yolk. Near the centre of this was a second and much smaller ring of blood spots, in the centre of which again was a small irregular reddish dot, about the size of the head of a pin. When examined under a low power this was seen to consist of a small, tortuous mass of vessels containing blood, contained in the interior of an apparently structureless and certainly shapeless mass of tissue. The batch of eggs which has just been described was examined on the eighth day.

(IV.)—This, which is the last series of experiments which I have made in this direction, gave results so unexpected that I am in doubt whether there may not have been some flaw in the arrangements. The eggs, twelve in number, were placed as in previous experiments, and removed for purposes of examination on the seventh day. Of the control specimens, three contained well-developed chicks, and one was not developed at all. Of the eggs connected with the battery, four contained well-developed chicks, and four were totally undeveloped, these being indiscriminately mixed with the rest.

It is obvious that these experiments must be carried out to a much greater length before any definite conclusions are arrived at. When the present investigation upon which I am engaged

is sufficiently advanced to permit of my using the incubator for that purpose, I intend to carry out a series of observations upon a much more extensive scale in the question of the influence of electricity on the development of the chick, and to use for this purpose both the continuous and the interrupted current.

3.—Influence of magnetism on development.

The influence of magnets in close proximity to eggs during their development is one which appears to me to require further investigation. The following observations have already been made. Maggiorani ([4]), during the process of artificial incubation, exposed a number of eggs to the influence of powerful magnets. A similar set of eggs, being hatched in the same manner, but kept away from all magnetic action, served as a check. Cases of arrested development were four times more numerous in the first group than in the second. Microscopical examination showed that the sterilisation of the germs was probably due to an intense vascularisation of the yolk-sac. After the birth of the chickens this increased mortality continued, deaths being three times more numerous in the magnetised group. All the counter test chickens reached their full development, whilst of the 114 of the first group sixty presented notable imperfections. Their movements were also abnormal. There were three cases of paralysis and two of contractions. Six of the chickens arrived at maturity. Of these two were cocks of a splendid stature, and endowed with an insatiable reproductive appetite. With the four pullets it was quite the contrary. One of them never laid at all, and the three others generally produced merely minute eggs (the heaviest weighing only thirty grms.) without yolks, without germinal spot, and. in a word, sterile. The magnetic influence upon the embryo is therefore evident, and its action upon the structure and the functions of the germ is still manifest when the latter has arrived at maturity. "May we not, to explain this effect of the magnets, suppose an interference between the magnetic vibrations and the heat vibrations which animate the molecules of the fecundated germ, and impel them towards a new condition of organic equilibrium. This

influence generally prevents, and more rarely retards, the development of the embryos (hypertrophy in the two cocks and atrophy in the four hens); and, as interference implies analogy, may we not infer that the vibrations which impel the germ towards its development are analogous to the magnetic vibrations?"

In connection with this last suggestion, and indeed with the whole subject of the influence of electricity and magnetism on the development of the egg, it is interesting to note that L. Hermann and A. v. Gendre ([5]) have found that on applying electrodes to the developing egg of a fowl there is an electric current passing from the yolk to the embryo, the latter therefore being positive towards the yolk. The current is so strong that it frequently suffices to drive the index off the field of vision. The electromotive power may reach as high a point at $\frac{1}{100}$ Daniell. It appears to increase in strength in the first stages of development, and reaches its maximum after about eighty hours; it then decreases. In view of these experiments, Mr. Slater ([6]) tried the effect of magnetic action upon the development of caterpillars. Having found six caterpillars of the common large white (cabbage), all evidently of the same brood, three of them were put in a box, 5in. in length, between the opposite poles of two bar magnets. The other three were placed in a similar box at such a distance that they could not be affected by the magnets. Both boxes were placed under exactly identical conditions as regards light, heat, and supply of food. Two of those between the magnets shrivelled up and died without passing into the pupa-state. Thinking they might have been attacked by some parasite, the author removed them into another box and kept them for some time. As no ichneumons or other parasites made their appearance, he dissected the bodies carefully under the microscope, and found no traces of parasitic injury. The remaining caterpillar, and all the three which were not exposed to the magnets, became pupæ in due course, and came out in May. The non-magnetised ones were perfectly normal and healthy, and when released after examina-

tion flew away; but the survivor of the magnetised set was a cripple. It had merely rudimentary stumps in place of antennæ, the wings on the left side were expanded, and the legs on the same side were smaller than on the right.

I have made myself only two sets of incubations under these conditions, and with negative results. It is, however, probable that the magnets which I employed, which were of very moderate power, may not have been sufficiently strong to exercise any influence upon the development of the embryo, and I therefore propose to repeat them with horse-shoe and bar magnets of much greater power.

Several other series of experiments having been purely tentative in their nature, and presenting no points worthy of present mention, are omitted wholly from this preliminary account.

Conclusions.—It may be well here to note a few points which the above-mentioned observations appear to lead to.

Firstly, they entirely confirm the view of Dareste, that the same anomalies may be produced by diverse methods. Writing on this subject, he says:—"Les mêmes anomalies peuvent être produites par les conditions physiques les plus différentes. Ainsi les quatre procédés tératogéniques que j'ai décrits dans le chapitre précédent, quelques différents qu'ils soient, m'ont donné cependant les mêmes faits tératologiques. Les anomalies que M. Panum a produites par l'emploi du refroidissement temporaire des œufs, et celles que M. Lombardini a produites par l'emploi de courants électriques, sont également de même nature que celles que j'ai produites par de tout autres procédés. En d'autres termes, il n'y a pas, le plus ordinairement du moins, de relation nécessaire entre l'emploi d'une certaine condition physique comme cause modificatrice, et une certaine modification de l'organisme."

But, in fact, I think we may go further than this, and say that whatever the disturbing agent may be which is employed, the failures of development are generally of the same nature: a consideration which, if true, as I believe, points to a much

deeper explanation of the variations and malformations produced than that which attributes them to the actual deprivation of air, or whatever the means may be which is used.

In the second place, I think there is now good evidence to prove that these disturbing agents act, at least in the great majority of cases, on that part of the developing organisation which is concerned with the formation of the vascular system of the embryo. I am inclined to think that it will be found that this is the case in every instance where the disturbing agent is really responsible for the malformation found in the egg submitted to its influence. It cannot be too carefully borne in mind that malformations may and do occur in eggs normally incubated or hatched by the hen, so that it is very rash to suppose that any malformation produced under false conditions is actually due to those conditions until its occurrence with extraordinary frequency and constancy under those conditions has been fully proved. There is, however, abundant evidence that the vascular system under any of the disturbing agents which have been employed undergoes remarkable and constant alterations. The illustrations in Panum's (7) work of these alterations agree in every respect with the appearances which I have frequently observed myself in eggs which have been incubated under all the conditions which I have employed, and many similar figures will be found in Dareste's book. Here, then, it appears there is a definite object for further study in the nature and history of the changes which take place in the vascular system. These changes I am at present engaged in working out, with the co-operation of my former pupil and assistant, Mr. C. F. M. Ward, now Demonstrator of Physiology in the Mason College. We are at present working with the method of varnishing the greater part of the surface of the egg-shell, as that seems to be the readiest way of obtaining the disturbances which we desire to study ; and, so far, we have been successful in obtaining a large amount of material on which to work. We hope to study the condition of the vascular system as affected by the various means commonly

employed for the disturbance of development, and to publish our observations when complete.

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IV.—*A Note on "Fixed" and "Occulting" Optical Apparatus for Lighthouses.*

By James Kenward, F.S.A., Assoc. Inst. C.E.

[Read before the Society, 3rd December, 1890.]

The Harbour "Fixed Light" of the Dioptric system of Lighthouse Illumination came, sixty-five years ago, from the hand of Augustin Fresnel in complete development. To quote the words of Mr. Thomas Stevenson,* "This was not only the first application of total reflection to lighthouses, but was the first optical combination which for the purpose required was both geometrically and physically perfect, leaving in fact no room for improvement; and accordingly this beautiful instrument continues till now in universal use."

Fresnel was unable, through default of men and machines in Paris, as also from failing health—he died in 1827—to apply his totally reflecting prisms to the lenses of his fixed sea-lights. This was afterwards done by Léonor Fresnel for the third order light of Gravelines (1842) and for first order fixed lights in the following year. Such prisms were also introduced, beneath the lenses, by Alan Stevenson, into the revolving light of Skerryvore (1844). This distinguished engineer had proposed in 1835 to

* *Lighthouse Construction and Illumination*, 1881, p. 68.

Léonor Fresnel that they should be used in first order fixed lights. To Alan Stevenson is due the credit of designing for fixed sea-lights the improved cylindric refractors, and the inclined lens-joints and lens-frames.

The Fresnel Fixed Light, as thus improved by Alan Stevenson, and subsequently with additional prisms, is indeed an optical instrument worthy of all praise. It is simple in its construction, and durable in its parts. It diffuses in every azimuth a full and steady beam of the whole height of the apparatus. It is excellently adapted to the intensification of light over particular sea-sectors by means of vertical condensing prisms, and to the sharp definition of coloured arcs by the same agents. It is the least costly of dioptric lights. It needs the minimum of skill in management. Thus in its various dimensions or "orders," it has been deservedly popular in all maritime countries.

But in comparison with a "Revolving Light," with annular lenticular elements and corresponding prisms, the "Fixed Light" has of course always been inferior in point of power or intensity of beam. The light condensed into successive periodic flashes, each subtending a certain horizontal arc, may be eight or ten times more potent than the light emitted from cylindric panels over the same arc. In comparison also with the uniformity and invariability of the Fixed Light, the Revolving is superior in its capacity of presenting several varieties in the intervals and duration of its flashes according to the breadth of the panels and to the speed of rotation imparted to them. It is true that colour may be used to vary the effect of a fixed light at the expense of at least one half its intensity; but colour may be equally used with a revolving light, increasing *pro tanto* the number of its characteristics. The illuminant, oil or gas, in perfected burners, may be applied as a common factor in all lights.

The ever-augmenting number and speed of ships, especially of steamers, in all the oceans and narrow seas, make it equally imperative that lights should be visible at greater distances

than of old, that their characteristics should be sooner recognised, and that their distinction from one another should be more and more unmistakable.

It is evident, then, that the simple fixed light, however equipped and appointed, would come into gradual disuse, and that recourse would be had in greater measure to flashing lights with their higher power and numerous modifications. In our country fixed sea lights are no longer erected, and but few in other countries, except where a blending of both principles is adopted in order to give a constant illumination nearer the shore—a doubtful benefit, however, for it must be at the cost of the illumination on the horizon.

Yet it is marvellous how in some places the fixed light survives as a sea-light, and in the most feeble form What more melancholy example than that recent one emphasised by the wreck of the Serpent! On the north-west coast of Spain, between Coruña and Cape Finisterre, are established two lights of the fourth order, that is, with optical apparatus about 30 inches high and 20 inches diameter. One of these, dating from 1853, is on the Sisargas Island, at an elevation of 351 feet above the sea. The other, dating from 1854, is on Cape Villano, at an elevation of 243 feet. Both of them are white fixed lights, but the Sisargas shows, in addition, a red flash at intervals of *four minutes*—an interval by far too long. These lights are charged with the duty of guiding ships along a portion of the dangerous Gallician coast. With such an elevation above the sea, the geographical range to the horizon would be 26 nautical miles in one case, and 22 in the other, that is, to a vessel's deck. These two headlands, therefore, are very suitable stations for a first or a second order light, but are very improper ones for a fixed light of the fourth order; the white beam from such an apparatus, even with a powerful lamp, not being trustworthy for more than ten or twelve miles, the red flash for not more than six or seven. What sort of flame was kept up in the lighthouses on that stormy 10th of November I know not. Probably each lamp was not, at the best, giving

more than 65 candles, which would not yield a beam exceeding 550 candles through the lenses—a poor intensity indeed for an apparatus with sea-light functions, and very likely to be diminished by the mist sure to cling around the headland. Unfortunately, too, no siren or other phonic signal was existing at either station.

When in a tempestuous sea, high wind and heavy rain, but with little or no fog, the ill-fated Serpent passed Cape Tosto, the Villano light should have been well visible at the very few miles' distance of the vessel; but no one on board appears to have seen it until just before she struck; nor is there any evidence that the Sisargas light had been previously seen. In these circumstances, is it too much to suggest that, while a really good fourth order fixed light should have sooner warned the Serpent of her dangerous proximity to the land, the presence of a modern light of revolving type with a range of, say, 20 miles, would have kept her safely out at sea after rounding Cape Adrian, despite the powerful currents that forced her to the east? It is stated that the Spanish Government will establish a light-ship off Cape Finisterre, and new lights on the capes Villano and Toriñana. It is none too soon.

I have said that the simple fixed sea-light has been well nigh withdrawn from the resources of the lighthouse engineer on account of the disabilities of inadequate power and inadequate distinctiveness. But in regard to the latter point a material modification has been effected, so that in those cases where the intensity is held to be sufficient, a very good distinction may be attained. This is the Occulting system, which converts the constant beam of a fixed light into an intermittent beam by means of an opaque shutter, cylinder, or screen moving at stated intervals over or around the flame of the lamp; or, in the case of a gas flame, by the sudden extinction and relighting of the flame itself. The French writers on lighthouses, whose clearness of expression and accuracy of definition are not the least valuable features of their textbooks, divide occulting lights into two classes: *feu scintillant*, where the duration of the

light is less than the duration of the eclipse ; and *feu clignotant*, where the duration of the eclipse is less than the duration of the light. I think, however, that this is not a perfect definition because in all flashing or revolving lights, properly so called, that is, where the optical apparatus itself rotates on its axis, the duration of the light is usually less than the duration of the eclipse ; yet the word "scintillating" would hardly apply here. But *feu clignotant* is a happy term for the true occulting light, which is properly, light interrupted by darkness—flashes of darkness, if I may so say—rather than darkness interrupted by light. In truth, there can be no more emphatic distinction in nature than darkness and light ; and the physical or metaphysical effect on the observer is clear and constant, as anyone may testify who has watched from the sea the quick alternation of beam and eclipse.

The methods of producing occultation, alike in principle, are various in detail. As far back as 1830, Mr. Robert Stevenson used, at the Mull of Galloway, two opaque drums, moved in opposite directions by clockwork, which masked and unmasked the reflectors vertically. At Point Lynas, in Anglesey, in 1835, horizontal shutters were made to open and close before the light, also a catoptric one. Professor Babbage, in 1851, suggested the use of an opaque shade, descending and ascending over the flame at unequal intervals, so as to indicate certain numbers or names referring to particular lighthouses. This rather embarrassing plan of rhythmical longs and shorts has been but rarely carried out in practice. In 1878 Messrs. Chance erected at Point Lynas, the storm-beat Point Elianus of the old geographers, a second order light with an occulting cylinder actuated by a spring clock, so that the flame was wholly and suddenly obscured for two seconds at intervals of eight seconds. The same engineers had, in 1876, adopted a like arrangement for a like occultation in their second-order light at St. Tudwal, Cardigan Bay. Several other sea-lights have been made by them with apparatus of similar design, the great feature being a complete obscuration by the dropping

cylinder. The most striking is that now under construction for the Oyster Island, Burmah, of which the lenticular portion is of 920 mm. radius, and of 80° vertical angle, with an occulting period of 28 seconds light and 2 seconds dark. The Trinity Corporation has been gradually giving the occulting character to their old fixed lights, thus in a large measure increasing the usefulness of them. "Eo magis elucet quo magis occultatur."

About 14 years ago, Baron Von Otter, of Stockholm, introduced occulting screens of a particular vertical section, opening and closing like the parlour-window blinds familiar to our childhood, and several foreign harbour lights have been made with this arrangement.

It is indeed in harbour and port lights that the essential utility of the occulting system is most evident. The multiplication of every variety of bright and coloured lights on the shore, on ships, and in houses, renders it very difficult for the navigator to approach a pier or an anchorage safely, to enter a dock or narrow channel, or even to keep the open sea in security. During the past few years there has been perfected at Spon Lane occulting apparatus exactly suited to these port and harbour conditions. The typical occulting port light of Messrs. Chance is composed of the central part only of the lenticular system, that is, the refractors, but extended to 80 degrees of vertical angle. It is made in three dimensions, the fourth, fifth, and sixth orders, or of 250, 187½, and 150 millimètres radius respectively. The specimen before you is of the sixth order, the smallest, composed of eight optical elements. It gives between the occultations, white steady beams over the whole horizon.

The lamp used is of about 35 candles, with a petroleum flame. It will burn untouched throughout the longest night. The intensity of the beam is about 200 candles. Around the lamp a set of black steel screens, fixed vertically on a frame, is rotated by means of a new and peculiar clockwork, having an effective governor. This clock needs rewinding but once every thirty-six hours. The width of the screens, and the interval between them, may be so arranged in connection with the size

of the flame, and the speed of the clock may be so regulated, as to produce occultations in various periods with the utmost completeness and precision. The eclipses may be single or in groups of two or of three, with different durations of light—for example, three seconds of light and two seconds of darkness, or eight seconds and two seconds, or seven seconds and three seconds which is the period of the apparatus before you. A mariner may, with great ease, know his position and take a bearing on watching the recurrence of these simple phenomena. The light as a white light is capable in this specimen of a range of at least eight miles. The faint gleam during the obscurations would be invisible at a short distance from the lighthouse. This present apparatus, with its copper lantern or framework, is destined for a position in the open air in an Italian harbour. In such a form perfect ventilation is secured, while all down-draught and up-draught is obviated. In general, the copper mountings are omitted when the position is in a chamber or large lantern.

There is a highly ingenious form of occulting light known as the "Trotter-Lindberg," in which the screens are rotated by the action of the flame of a lamp within the optical apparatus, the illuminant being a very volatile spirit called, I believe, gasolene. There is an example of such a light at Stoneness, on the Thames. I doubt, however, whether the defined periods of light and darkness can recur with the needful precision under the varying temperature of the flame.

The rotating screens or the dropping cylinder may be always used when the illuminant is oil, but when gas is the aliment of the central flame, a more advantageous method is to turn the gas off and on, in sequence of light and dark, according to the periods chosen, which is easily effected by adapting the clock to the purpose, or it may be effected by making the flow of the gas produce automatic intermittent action by means of a dry meter, as first suggested by the late Thomas Stevenson.

Mr. Wigham, of Dublin, has introduced, *multâ cum laude*, the mechanical cutting off of the gas into some fine sea-light

on the Irish coasts, and it has been with equal success carried out by Messrs. Chance in apparatus at home and abroad. They also designed, in 1883, a unique specimen of an occulting floating light, consisting of *three* dioptric apparatus of the fifth order, the three occulting cylinders being actuated by one clock, the time being ten seconds of light and four seconds of darkness. This was erected in a lightship at Proudfoot Shoal, Queensland.

Perhaps the most completely equipped occulting light existing is that one of the Messrs. Chance constructed last year, under the superintendence of the writer, for the new Barry Docks, near Cardiff. This is a refracting apparatus of the fourth order, set on an iron tower 30 feet high. It is white, and occulting with a period of five seconds ($3\frac{1}{2}''$ light, $1\frac{1}{2}''$ dark); and it is suited equally to the three illuminants of gas, oil, and electricity. Owing to the isolated condition of the site, the clockwork is arranged to go for eight days without rewinding; and also, when gas is used, to start and stop the light, evening and morning, at times varying with the seasons. During the exhibition of the light with a gas flame, the gas is turned off and on automatically; while with the oil flame, or with the electric glow lamp, an occulting cylinder is employed, both movements being effected by the clock. The intensities of the glow lamp, the gas flame, and the petroleum flame are equalised very nearly, so that the beam sent out to sea is always of uniform power.

Not long ago I had the pleasure to submit to this Society, an account of the greatest lights of the Dioptric system, the hyper-radial apparatus of 1,330 millimètres of focal distance, a thousand times exceeding the power of the little light before you; and also an account of the new signal lights which have proved of such eminent benefit to steam vessels. May I hope that the present apparatus, a specimen of a type devoted to the multifarious service of the world's ports and harbours, will not be without interest in your eyes?

V.—*The Evolution of the Earth's Crust.*

By Charles Callaway, D.Sc., M.A., F.G.S.

[A special address delivered before the Society on Dec. 3rd, 1890.]

In the retiring address of your late president, Dr. Deane, there is a reference to the theories which have been advanced to account for the present constitution of the earth's crust; and it is concluded, rightly I think, that the principle of evolution is as applicable to the construction of the earth-floor as to the animals that walk over its surface. Yet the geologist who has devoted himself to the study of the foundation-rocks of the globe, may be pardoned if he permits himself an amiable toleration for the errors of the catastrophists. The further back we penetrate into the recesses of Archæan time, the more strongly marked become the traces of those potent forces that determined the structure of the primæval crust. We are like the early explorers into the mysteries of the Pyramids of Egypt, who beheld before them, in the dim chambers, the mummied remains of the mighty monarchs whose degenerate descendants sway delegated sceptres. The Titanic laws of the early world are still with us; but they work at greater depths, and the sound of their operations rarely reaches us. Mechanical, thermal, and chemical energies every now and then remind us of their existence. Explosions that blow out the side of a mountain, or shatter a group of islands into heaps of ruin,

are catastrophes that would have satisfied De Beaumont himself. In Tertiary times, the fire-forces rolled up the strata of Central Europe into a complex of loops and twists. The North American region witnessed enormous upheavals and contortions of the crust at more than one interval during the Mesozoic period. The earth-movements of the Palæozoic ages were on a gigantic scale. In our own Highlands mechanical energy seemed to run riot. The solid earth-crust was compressed so strongly by lateral force that it gave way again and again, and slices of rock as large as Staffordshire were sent sliding uphill for miles. These moving counties went grinding on over extensive formations of sandstone, quartzite, and limestone, crumpling up the massive strata like so much tissue paper, or converting the sediments into the likeness of crystalline schists. But it is in the older Archæan rocks that the catastrophist would be in his glory. Here the Titanic horse-play of Pluto's myrmidons is at the height. In the younger epochs, the crushing up of a mass of the crust into a mountain chain is an occasional incident; but in the Lower Archæan rocks the signs of enormous pressures are more common, and the alterations produced are carried to a more advanced stage.

I need hardly point out that the apparent catastrophism of these remote epochs is the strictest uniformitarianism. The crushing up of a solidified granite into a gneiss may be taking place at the present time at more than one point of the earth's crust. Differential movements, caused by unequal stresses acting at great depths during the process of cooling, may produce at the surface no visible effect beyond secular upheaval or depression. However, I do not care to insist upon this. I desire merely to emphasize the obvious fact, that the natural forces acting during the earliest epochs are still existing, and would produce the same effects if it were possible to bring them into the same environment.

But the environment is not the same; it changes from age to age. As secular cooling proceeds, the earth-crust grows thicker and more rigid, the fire-forces are pent within narrower

limits, and lateral compression, resulting in flexure and crushing, is feebler in quantity, and has to overcome greater resistances. The changed environment has compelled an evolution. Heat and water have been struggling for supremacy. At first, heat was the mightier force. Then it withdrew deeper and deeper into the crust, and water acquired the dominion. In the water, life was generated, and evolution culminated in man.

In discussing the evolution of the earth's crust, I desire to confine myself to the earlier epochs. When we get into the Palæozoic periods, we are like Stanley when he emerged from the jungles of the great Congo Forest, and walked with free step over the grassy table-land above Lake Albert Nyanza. Amongst these sedimentary rocks there are few disputed successions, and hardly any insoluble problems. But we Archæan geologists are still in the jungle; and the advanced parties often lose precious time in fetching up the rear columns. Which are the advance, and which are the rear, I will not presume to say.

You will have inferred from my last sentences that the students of the oldest rocks are not agreed amongst themselves. They are not surprised at this who are familiar with the imperfection of our material and the obscurity of the problems in hand. The areas of the Lower Archæan rocks are usually mere pavements, formed of fragments thrown together pell-mell in a chaos of confusion, then partly remelted, perhaps cemented together, and crushed up by forces acting from several directions in succession, and last of all planed off to a comparatively level surface. In such a field of work as this, he that runs ought *not* to read; but, if he does, his published results need not be read by others.

Nevertheless, there are a few great generalisations which have met with considerable acceptance amongst those who have paid the most prolonged attention to these old rocks. At the base of the entire sedimentary series are great masses of crystalline matter, known by such names as the Fundamental Gneiss, the Laurentian, the Hebridean, the Malvernian, and

perhaps the Dimetian. In the British area, these rocks appear in the Hebrides and North-Western Highlands, in Anglesey, in the Malvern Hills, in Shropshire, and perhaps at St. Davids. It is also probable that a large portion of the crystalline schists of the Lizard, the Channel Islands, Donegal, and Connemara, will go into the Lower Archæan. It is generally agreed that these fundamental masses are succeeded by a great succession of volcanic rocks, first recognised at St. Davids by Salter and Hicks, and named "Pebidian" by the latter. The probable equivalent of this formation in Shropshire is the series of lavas and ashes which I have called "Uriconian." The well-known volcanic rocks of Charnwood Forest, so well studied by Professor Bonney and the Rev. E. Hill, are probably of the same age. In North Wales, the Pebidian has been recognised near Caernarvon and Bangor, and in parts of Anglesey. I believe they also occur at Howth, near Dublin, and around Wexford; but in these districts the volcanic facies is not so strongly marked. I do not here discuss the rocks called "Arvonian" by Dr. Hicks, or the "Monian System" of Professor Blake, for the reason that these supposed formations have not yet been recognised as realities by Archæan geologists.

There is a third rock-group which will fall within the scope of my discussion. I refer to the great succession of sediments known as the "Longmynd Series," or the "Longmyndian." Whether these rocks should rightly be referred to the Archæan, I do not here discuss; it is sufficient for my purpose to regard them as intermediate in age between the Uriconian epoch and the quartzite which in Shropshire forms the base of the Cambrian series.

In these three groups we have types of three kinds of rock-formation—the plutonic, the volcanic, and the sedimentary, and they succeed each other in the order named. In so far as I shall base a theory upon my studies of these rocks, I desire distinctly to limit the application of that theory to the British area.

We commence with the basement rocks—the Hebridean or Malvernian gneisses and schists. How did these originate?

They are crystalline, and they frequently present the appearance of a stratified formation. Until recently nearly every geologist believed that they were ordinary sediments which had been changed to the crystalline state by heat and pressure acting at great depths. If they were sediments of this kind, they were obviously derivative from pre-existing rock-masses, and therefore could not form the first term in an evolution. They might be the second stage, the third, the fifth, the tenth, in the great process. But if we can prove that these rocks are not derivative, that they are composed of materials similar to those which now underlie the stratified portion of the earth's crust, and which probably formed the crust before any sediments were laid down, we arrive at a theory of the state of the globe in Lower Archæan times, which has some approach to definiteness. I believe that such a theory can be applied, at any rate within the limits I have defined.

For several years I have been studying, in the field and the microscope, the older crystalline schists forming the ridge of the Malvern Hills, a considerable part of the island of Anglesey, and two small patches in Shropshire; and I have come to the conclusion that in all these districts the apparently bedded structure in the rocks has been produced in crystalline igneous masses by enormous pressure. Without further preface, I will briefly outline the theory of origin.

We start with a solidified crust composed of a few varieties of massive igneous rock interlacing each other irregularly Three of these will suffice for our purpose—granite, diorite, and felsite. The granite is usually a coarsely crystalline compound of quartz and a potash-felspar of a brick-red colour. Mica, which forms a third constituent in normal granite, is inconspicuous or wanting. Diorite is in several varieties. It is coarse or fine-grained. It is normal in character, being chiefly composed of green hornblende and one or more varieties of soda-lime felspar. The felsite is also of more than one kind, the felspar in some localities containing soda in prominent proportion, in others potash.

A gneiss or schist may be produced out of any one of these igneous rocks. Let us, for example, take granite. As a cook handles a shapeless lump of paste, and rolls it out into layers, in which he produces a minutely flaky structure, so earth-pressure squeezes a mass of solidified granite into a band of gneiss, in which the constituents are arranged in parallel flakes or folia. The squeezing force is so great that a differential movement is set up in the solid mass, particle sliding over particle, and flake over flake, so that there is a flow of the rock as real as if it were in the fluid state. Important mineral changes accompany the deformation of the mass of granite. The felspar is partly decomposed into quartz and mica, and some of it is reconstructed in smaller crystals. The quartz also may be redeposited in a finely granular form. The product of the crushing process is a rock composed of quartz, felspar, and mica; these constituents being arranged in thin flakes or leaves. Such a rock is a true gneiss, and it has been produced out of granite by pressure.

In like manner diorite may be converted into a hornblende-gneiss, a chlorite-gneiss, or even a mica-gneiss; and felsite may be changed into mica-gneiss or mica-schist.

The intermixture of two or more kinds of rock, as granite and diorite, or felsite and diorite, gives rise to gneisses of a very stratiform appearance. Imagine a mass of the dark-green diorite penetrated by numerous veins of grey granite, and then rolled out in the earth-mill. The pressure produces a general parallelism amongst the veins, and the result is a rock made up of parallel bands of a green and grey colour. We cannot wonder that the older geologists took these bands for layers of original sedimentation, and described the rock as of aqueous origin.

I have briefly sketched an outline of the igneous theory of the genesis of the older gneisses and schists. On the mineral and chemical changes involved in the metamorphism I have had no time to enlarge, and barely to touch. I have communicated two papers on the subject to the Geological Society of London, and a third is nearly ready. In these will be found the proofs

of the theory. I may add that similar conclusions have been reached by the members of the Geological Survey with respect to the Hebridean rocks of Scotland; and by Mr. Teall in reference to the Lizard district. Your own member, Professor Lapworth, is one of the forerunners in this field of investigation in Britain, and has published results which, in a high degree, combine precise definition with bold originality.

But if it be granted that the Lower Archæan rocks within the area I have defined are of igneous orign, it does not follow that the fundamental gneisses of North America, the Alps, Bohemia, and other regions, are to be explained in the same way. Nevertheless, the labours of some of the most recent investigators in North America, and on the Continent of Europe. point towards a similar result, and the old arguments in favour of the sedimentary hypothesis are being seriously impugned. I will briefly notice some of these.

It has been contended that the old gneisses must be altered strata, because they contain organic limestones. The alleged organic remains are the famous *Eozoon Canadense*, and similar structures. Personally, I entirely disbelieve in the organic nature of this so-called fossil. But *Eozoon* is of American birth and citizenship, and its own fellow-countrymen have had the best opportunities for studying its true origin. Two or three years ago a jury of American experts, authorised by the American Committee of the International Geological Congress, sat upon the case, and they decided by a vote of ten to four that *Eozoon* was not organic. One of the voters on each side had no independent opinion, so that the numbers were more accurately nine to three. Of the three, one was not certain. The two remaining were Dr. Sterry Hunt, and Sir William Dawson, the originator of *Eozoon*. On the other side, eight speak positively, and one, Professor Wadsworth, declares that "it has been proved to be geologically impossible that it (*Eozoon*) is organic." I fear, therefore, that deeply as we may regret the exclusion of the pretty pictures of *Eozoon* from our text books, the sacrifice must be made.

If *Eozoon* be consigned to limbo, the organic origin of the Laurentian limestones becomes mere assumption. On the other hand, certain Lower Archæan limestones which I have studied in Anglesey can, I think, be proved to be mere segregations. The rock of the district is a mica-schist, which has been formed out of a soda-lime felsite by pressure. During the crushing, chemical dissociation supervened, the bases of the silicates being partly separated from the silica. The lime thus liberated was converted into carbonate by meteoric waters, and aggregated into masses, which under the pressure took the form of lenticular bands. No limestone beds are known elsewhere in these rocks in Britain; and whether the explanation will apply to the ancient limestones of Ireland, Canada, and elsewhere remains to be seen.

The presence of graphite and iron-ores has also been urged as proof of the existence of organic remains in the Lower Archæan periods. It does not however follow that, because these substances are sometimes connected with the action of living plants, there is no other explanation of their origin. As to iron-ores, my studies in the Malvern rocks have shown me conclusively that iron-ores may be formed in the heart of igneous masses. Thus, hornblende, when it is decomposed, yields a powder of iron-oxide. This powder, under a reconstructive process, aggregates into definite crystalline forms as magnetite. If this ore may be formed on a small scale without the aid of plant life, why not in larger masses? That graphite must always have been formed from plants is a pure assumption, for which I contend there is no evidence whatever.

But while I submit evidence hostile to the alleged existence of fossils in the Lower Archæan rocks, I must not be understood to urge the igneous theory beyond the limits originally defined. I think it probable that all these foundation-rocks throughout the globe are of igneous origin; but this is a mere suggestion, and time may refute it.

The next rock-system in the British area is still igneous but it is volcanic, not plutonic. The gneisses and schists of

the older group were formed at some depth in the crust, the Uriconian lavas and ashes were erupted at the surface. It would seem probable that volcanic conditions set in as soon as the crust had congealed, and it may be that gneiss formation and volcanic eruption were going on at the same time. If this were so, the traces of these older Archæan volcanoes have not been preserved to us. The ejecta of the Uriconian volcanoes are clearly of a younger epoch, for they have not been exposed to the enormous lateral pressures of the older rocks, and they contain conglomerates whose well-rounded fragments of gneiss and schist are derived from rocks which were formed far below the surface. There must therefore have been a period of upheaval and denudation previous to the laying down of these conglomerates.

It is needless for me to enter into a detailed account of the Uriconian series. The most abundant rocks are lavas, ashes agglomerates, and volcanic mud. The thickness must be very considerable, but no summit or base is known. The materials do not essentially differ from the ejecta of modern craters. The volcanic conditions must have been widespread, for they are found at points as far apart as Charnwood, St. David's, Shropshire, Anglesey, and Dublin. This area probably might be extended much further, for similar rocks are found below the Cambrian in France, Scandinavia, South America, India, and elsewhere.

The third of the rock-systems under discussion, the Longmyndian, is purely sedimentary. Conglomerates and grits alternate with shaly and slaty beds. The thickness must have been enormous. The Geological Survey estimated it at nearly five miles, and no top or bottom to the succession is known. But though differing so widely from the Uriconian in the conditions of deposit, there are marked resemblances in the mineral composition. The slates of the Longmynd are not unlike the volcanic muds of the older series, and the grits are like the grits. The Longmyndian, however, contains a fair proportion of fragmental mica, a mineral which is almost wanting

in the older group. If the lavas of the Uriconian were omitted, and the clastic material were laid out in even beds instead of irregular patches, the two formations would differ mainly in the presence and absence of the mica. I infer from this that the materials in both cases came in great part from similar sources. I think the sediments of the Longmyndian were derived partly from Lower Archæan crystalline masses, partly from the denudation of the Uriconian, and partly, perhaps, from distant volcanic centres whose finer ashy material was carried by currents of water into the Longmynd area.

I do not attempt to carry the evolution any further. From the Cambrian epoch onwards the succession of rock-systems is, in the main, intelligible and undisputed. I have dwelt upon some of the earlier chapters of earth-history, because our knowledge of these obscure epochs is but slight, and any gleams of light will, I trust, be welcome.

I have said nothing of the evolution of organised life during Archæan times. I am in the position of that worthy mayor who did not ring the town-bells at the coming of a great potentate, and who urged as his excuse that he had no bells to ring. The Malvernian gneiss can never yield us fossils, and the Uriconian volcanic rocks are well-nigh as hopeless. But the fine-grained sediments of the Longmyndian seem to be the natural habitat of myriads of organic remains. Yet, if they are there, no one has found them. A fragment of a trilobite, *Palæopyge Ramsayi*, has been quoted from Longmyndian slates; but this unobtrusive crustacean has hitherto failed to convince the world of its existence. These slates have, however, yielded traces of the existence of animals that crawled over smooth surfaces of fine mud, leaving behind them either continuous tracks, or shallow pits, such as might have been made by the feet of crustaceans. But whatever these inhabitants of the shores of the Longmyndian seas were like, no solid fragment of their bodies has been detected. Yet we can hardly doubt that the world was already peopled by a considerable variety of plants and animals. In the next epoch, the Cambrian, life was

abundant. At the very base of that great series are found trilobites, brachiopods, and annelids—animals of a highly complex structure—and these, if evolution is true, must have been preceded by countless generations of simpler types. We seem almost bound to believe that the Longmyndian period was one of comparatively abundant life. What then has become of the numerous ancestors of the Cambrian fauna? Were all the animals soft-bodied? Were the molluscs furnished only with a tunic, and the crustaceans with a tough epidermis? And was the lack of shell and crust due to a want of lime in the sea? It is at any rate a suggestive fact that in the entire Longmyndian series no limestone, so far as I know, has yet been found. Limestones occur in the Pebidian rocks of Anglesey, but they are probably of chemical origin, and may have been formed in fresh-water. But who shall say how many formations below the Cambrian have once existed? The gap between the Malvernian and the Uriconian epochs may have been enormous, and may cover more than one rock-group. The unconformity between the Uriconian and the Longmyndian may also be considerable. Then, too, who will venture to affirm that whole formations representing the hiatus between the Longmyndian and the Cambrian have not been swept away? The problem of discovering the earliest forms of life is, therefore, one which will be hard to solve, if it is ever solved at all.

I have endeavoured, in this sketch, to confine myself to matters falling within my own observation. I have not ventured to intrude into the domain of the mathematician and the physicist. Questions affecting the cause of movements in the earth's crust have been investigated with considerable success by your secretary, Mr. Davison, by Mr. Mellard Reade, and by the Rev. Osmund Fisher. The determination of a level of zero-pressure, worked out, I believe, independently by Mr. Mellard Reade and Mr. Davison, is one of the most important of recent contributions to the physics of the earth's crust. This region of investigation is, however, one in which there is still much to be done.

Perhaps I may be allowed, in conclusion, to congratulate this society, and this city, on the extreme richness of what I will venture to call its Geologozoic fauna. I need but mention the names of Allport, Lapworth, Crosskey, Deane, Davison, Mathews, Waller, Houghton, and Harrison, to convince you that geological science, in nearly all its branches, holds a high place amongst the intellectual activities of Birmingham. This fauna, I am glad to say, is by no means extinct; and, being eminently fit, I hope it will long survive.

VI.—*Some Relations of Electromotive Force to Atomic Weight, Chemical Heat, etc.*

By Dr. G. Gore, F.R.S.

[Read before the Society on February 11th, 1891.]

The following experiments and measurements were made for the purpose of examining and extending the knowledge of the relations of volta electromotive force to atomic weight, etc. All the measurements of electro-motive force were made by the method of balance, with the aid of a suitable thermoelectric pile (see "Proceedings Birmingham Philosophical Society," Vol. IV., p. 130; "The Electrician," 1884, Vol. XII., p. 414), and a differential astatic galvanometer having 50 ohms resistance in each coil.

In order to ascertain the influence of variation of the kind of positive metal of the voltaic couple, nearly all the measurements were repeated with four different positive metals. The proportions of substances employed were in all cases chemically equivalent to each other. The proportions of water and of absolute acid, stated in grains, in all the experiments of Table 2, were as follows:—Water 18·0, HI 1·28, HBr ·81, HCl ·365, HNO^3 ·63, and H^2SO^4 ·49; but the actual quantities used were, for the sake of convenience, twelve times these amounts. The

hydriodic acid was freshly prepared, colourless, and highly pure. Distilled water was used in all cases. In consequence of the great voltaic energy of the halogens, and the small degrees of solubility of iodine and chlorine in water, the solutions of Table 1 were made just one 250th the degree of concentration of those of Table 2; they were, however, of equivalent strength amongst themselves; those of Table 3 were also chemically equivalent to each other. In all the tables the variation of electromotive force due to difference of temperature (6° to 12° C.) was small.

The means employed for obviating undue influence of polarisation in these experiments have been already described ("Philosophical Magazine," December, 1890, p. 484), and were effectual. Nitric acid solutions were the least satisfactory, because they were apt to produce films upon the positive metal. For greater convenience of manipulation, the voltaic couple was raised out of the exciting liquid, and lowered into it by means of a rack and pinion.

All the experiments and results in each of the tables are so arranged as to facilitate comparisons of the amounts of electromotive force with the atomic weights and amounts of chemical heat of formation and solution of the compounds. In each of the tables, the substances are classed in the usual chemical groups, for it had been found that by arranging them in different ways from this, the relations of the amounts of electromotive force to the atomic weights and chemical heats were obscured and could not be detected; in each group the members are arranged in the order of their atomic and molecular weights, *increasing downwards*. In the upper part of Table 2, the substances of each group are placed according to the atomic weights of the halogens, and in the lower part according to those of the metals. The amounts of heat given are those determined by J. Thomsen, as produced by the chemical union (and solution) of the elementary substances, and are stated in centigrade gramme calorics. The degrees of electromotive force are given in volts. In all cases, the order

of *increase* of the numbers is indicated by the direction of the arrows. In order not to make Table 2 too bulky, the relative losses of electromotive force are omitted; they may be obtained by subtracting the electromotive forces of the salts from those of their respective acids in the same table, or in the case of bromides from those of bromine in that table. From the relations and proportions of the numbers in Table 1 to those in Table 2, some conclusions may be drawn respecting the approximate amounts of the losses of electromotive force occurring during the chemical union of chlorine, bromine, and iodine, with equivalent weights of hydrogen and with the metals of the various salts employed.

TABLE 1.

(Weak Solutions.)

Electromotive Forces and Losses of Electromotive Force of Halogens by Chemical Union with Hydrogen.

	Units of Heat.	Sn+Pt.		Al+Pt.		Cd+Pt.		Zn+Pt.	
		E.M.F.	Loss.	E.M.F.	Loss.	E.M.F.	Loss.	E.M.F.	Loss.
Cl ...		1·27278		1·5959		1·6508		1·9421	
Br ...		1·19270		1·4672		1·5135		1·8706	
I ...		·89240		1·1240		1·1646		1·5274	
HCl ...	39,315	·73796	·53482	·9953	·6006	1·0130	·6378	1·3558	·58630
HBr ...	28,380	·70936	·48334	·94954	·51766	·9872	·5263	1·30718	·56342
HI ...	13,170	·65788	·23452	·8666	·2574	·88432	·58028	1·17562	·35178

The proportion of loss varied from 22·90 to 42·02 per cent.

TABLE 2.

(Strong Solutions.)

Electromotive Forces of Acids and their Salts.

Substance.	Units of heat.	Sn+Pt. Volts.	Al+Pt. Volts.	Cd+Pt. Volts.	Zn+Pt. Volts.
Br ...		1·2213	1·5816	1·6222	2·0136
H^2SO^4 ...	142,410	·8209	1·08396	1·1646	1·38726
HNO^3 ...	70,665	·8352	1·08320	1·1330	1·4702
HCl ...	39·315	·8086	1·2986	1·1215	1·46734
HBr ...	28,380	·7980	1·0954	1·11026	1·3844
HI ...	13,170	·7351	·9667	1·0931	1·3129
AmCl ...		·7065	·93810	·9146	1·2557
AmBr ...		·68362	·89520	·8786	1·18134
AmI ...		·67504	·86374	·87288	1·0984
NaCl ...	193,020	·7351	·9953	·99586	1·2557
NaBr ...	171,160	·73796	·95526	·8786	1·1842
NaI ...	140,600	·6922	·89806	·8786	1·1413
KCl ...	202,320	·7494	1·0096	·98728	1·2271
KBr ...	180,460	·7494	1·0382	·8956	1·1985
KI ...	150,040	·68934	·9095	·88164	1·16418
RbCl ...		·75512	1·0096	·93008	1·2308
RbBr ...		·7351	·9810	·90148	1·20708
RbI ...		·6779	·8809	·88432	1·1699

TABLE 2—(*Continued*).

(Strong Solutions.)

ELECTROMOTIVE FORCES OF ACIDS AND THEIR SALTS.

Substance.	Units of heat.	Sn+Pt. Volts.	Al+Pt. Volts.	Cd+Pt. Volts.	Zn+Pt. Volts.
LCl ...	204,500	·6922	·9095	·9787	1·1842
LBr ...	182,620	·66932	·8666	·8386	1·03548
LI ...	152,200	·65216	·8523	·8071	1·0126
$CaCl^2$ ...	187,230	·6779	·9667	·95868	1·17562
$CaBr^2$ ...	165,360	·7208	·9381	·9215	1·0412
CaI^2 ...	134,940	·66932	·8566	·88718	·9982
$SrCl^2$ ...	195,690	·6779	·9667	·9644	1·1985
$SrBr^2$ ...	173,810	·6922	·8666	·83856	1·17848
SrI^2 ...	143,460	·6636	·8523	·81568	1·02976
$BaCl^2$ ...	196,810	·6779	·9667	·97298	1·20136
$BaBr^2$ ...	174,940	·67218	·8237	·81568	1·18992
BaI^2 ...	144,520	·65216	·8094	·79852	1·04406
$MgCl^2$ ...	186,930	·66932	·95812	·96726	1·16132
$MgBr^2$ ...	165,050	·6922	·8952	·86716	1·04406
MgI^2 ...	134,630	·66074	·8694	·8585	1·0698
$ZnCl^2$ ...	112,840	·66646	·9524	·96726	1·17216
$ZnBr^2$ ...	90,960	·69792	·89306	·88432	1·1742
ZnI^2 ...	60,540	·6636	·86088	·87574	1·1556

TABLE 2—(*Continued*).

(Strong Solutions.)

Electromotive Forces of Acids and their Salts.

Substance.	Units of heat.	Sn+Pt. Volts.	Al+Pt. Volts.	Cd+Pt. Volts.	Zn+Pt. Volts.
$CdCl^2$ ...	96,250	·66360	·9238	·9644	
$CdBr^2$ ...	75,600	·72080	·9095	·9358	
CdI^2 ...	47,870	·66932	·8523	·9215	
NaCl ...	193,020	·73510	·9953	·99586	1·2557
KCl ...	202,320	·7494	1·0096	·98728	1·2271
RbCl ...		·75512	1·0096	·93008	1·2308
$CaCl^2$ ...	187,230	·6779	·9667	·95368	1·17562
$SrCl^2$ ...	195,690	·6779	·9667	·9644	1·19850
BaCl ...	196,810	·6779	·9667	·97298	1·20136
MgCl ...	186,930	·66932	·95812	·96726	1·16132
$ZnCl^2$ ...	112,840	·66646	·95240	·96726	1·17216
$CdCl^2$ ...	96,250	·66360	·92380	·96440	
NaBr ...	171,160	·73796	·95526	·8786	1·1842
KBr ...	180,460	·74940	1·03820	·8956	1·1985
RbBr ...		·73510	·98100	·90432	1·20708
$CaBr^2$ ...	165,360	·72080	·93810	·92150	1·0412
$SrBr^2$ ...	173,810	·69220	·86660	·83856	1·17848
$BaBr^2$ ...	174,940	·67218	·82370	·81568	1·18992

TABLE 2—(*Continued*).

(Strong Solutions.)

ELECTROMOTIVE FORCES OF ACIDS AND THEIR SALTS.

Substance.	Units of heat.	Sn+Pt. Volts.	Al+Pt. Volts.	Cd+Pt. Volts.	Zn+Pt. Volts.
$MgBr^2$ ...	165,060	·69220	·89520	·86716	1·04406
$ZnBr^2$ ...	90,960	·69792	·89806	·88032	1·17420
$CdBr^2$ ...	75,640	·72080	·9095	·93580	
NaI ...	140,600	·69220	·89806	·87860	1·14130
KI ...	150,040	·68934	·90950	·88164	1·16418
RbI ...		·6779	·8809	·88432	1·16990
CaI^2 ...	154,940	·66932	·8566	·88718	·99820
SrI^2 ...	143,460	·66360	·8523	·81568	1·02976
BaI^2 ...	144,540	·65216	·8094	·79852	1·04406
MgI^2 ..	134,630	·66074	·86374	·85850	1·06980
ZnI^2 ...	60,540	·66360	·86088	·87574	1·15560
CdI^2 ...	47,870	·66932	·85230	·92150	
Na^2SO^4...	257,970	·70364	·91808	·99300	1·2700
K^2SO^4 ...	267,180	·71794	·92380	·96440	1·29002
Rb^2SO^4...		·73510	·92952	·94438	1·30146
$MgSO^4$...	251,510	·7065	·9238	·9787	1·16990
$ZnSO^4$...	177,420	·70364	·9238	·9787	1·15560
$CdSO^4$ ...	161,210	·70078	·9095	·96726	

TABLE 2—(*Continued*).

(Strong Solutions.)

Electromotive Forces of Acids and their Salts.

Substance.	Units of heat.	Sn+Pt. Volts.	Al+Pt. Volts.	Cd+Pt. Volts.	Zn+Pt. Volts.
$NaNO^3$...	216,450	·68016	·9095	·98442	1·17216
KNO^3 ...	225,930	·68362	·9095	1·0073	1·17848
$RbNO^3$...		·68648	·91236	1·01016	1·19278
Average		·639576	·91513	·94586	1·18453

Conclusions.—Comparisons of the orders of the various series of numbers in Table 2 show—1st, that the amount of chemical heat varied inversely as the atomic weight in 14 series and directly in 8, or in a proportion of 1·75 to 1·0. 2nd, the amount of electromotive force varied inversely as the atomic weight in 49 series and directly in 18, or 2·72 to 1·0. 3rd, the electromotive force therefore was similarly, but more uniformly related than the chemical heat to the atomic weight. 4th, the exceptional cases in which the electromotive force most often varied directly as the atomic weight, viz., the sodium and calcium groups of metals, were also often those in which the chemical heat varied directly as the atomic weight, *i.e.*, the two conditions frequently varied together. 5th, the electromotive force varied directly as the chemical heat in 51 series and inversely in 13, or 3·92 to 1; this result, and the immediately previous one, confirm the conclusion, that the amounts of electromotive force and of chemical heat are closely connected together. 6th, as the amount of electromotive force varied more uniformly with that of chemical heat than with the magnitude of the atomic weight, it appears to be more directly

related to the former than to the latter. 7th, as the electromotive force is manifestly related to the atomic weight, it is probably connected with the periodic series; and 8th, the electro-motive forces of the halogens exhibited a more uniform relation than those of either of the groups of metals, to the atomic weights and chemical heats, probably in consequence of the greater energy and more conspicuous differences of energy of the former than of the latter substances. The weakest halogen acid, hydriodic, usually lost the most electromotive force by neutralisation. As both the electromotive force and the chemical heat depend upon each of the two substances which unite together, they are neither of them likely to vary uniformly with the atomic weight of one of the uniting substances alone, nor with that of the other alone, but in a less simple manner.

Usually the electromotive force of a voltaic couple is greater, the larger the chemical heat, the smaller the atomic weight, the less the specific gravity, and the greater the specific heat of the halogen and metal which chemically unite to produce the current; but in the case of the sodium and calcium groups of metals more particularly, the second of these conditions is often reversed; the metals of these two groups are those which have the smallest number of atoms in a given bulk. An examination of the *losses* of electromotive force of Table 3 shows, as might be expected, a less distinct relation to the atomic weight and amount of chemical heat than the electromotive forces themselves. The average loss of electromotive force by neutralisation of all the various acids was with Sn ·1052; Al ·1908; Cd ·1901; and Zn ·2388 volt.

The percentages of loss of electromotive force by dilution, which occurred on employing the weaker solutions of Table 1 instead of those of Table 2, which were 250 times stronger, were as follows:—

TABLE 3.

Losses of Electromotive Force by Dilution.

	With Sn. Per cent.	With Al. Per cent.	With Cd. Per cent.	With Zn. Per cent.
Br ...	2·35	7·24	6·71	7·10
HCl ...	8·74	23·36	7·60	7·60
HBr ...	11·11	13·32	11·08	5·58
HI ...	10·51	10·36	19·10	10·46

Diluting the stronger solutions to one 250th part of their strength had therefore comparatively moderate effects upon the degree of electromotive force.

VII.—*Modern Developments of the Atomic Theory.*

By William A. Tilden, D.Sc., F.R.S.,

Professor of Chemistry and Metallurgy in the Mason Science College, Birmingham.

[Read before the Society, March 18th, 1891.]

An enquiry into the nature of the evidence upon which a belief in the atomic constitution of matter was based by Dalton leads to the conclusion that in the first instance, and for many years, the hypothesis was supported only by a very small number of well-established facts. Dalton himself seems to have shown as early as 1802 that oxygen "may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity."* Subsequently his investigations of olefiant gas and light carburetted hydrogen led him to regard these two compounds as formed of carbon and hydrogen only, the latter containing twice as much hydrogen as the former, with the same proportion of carbon. There is, however, no formal statement in any of Dalton's writings of the course of experiment and reasoning which led him to formulate the views of chemical combination which are so clearly expressed in his "New System of Chemical Philosophy," and which represent even at the present day almost all that can be regarded as established in this direction. He says (Part I., p. 212):—"Chemical analysis and synthesis go no further than to the separation of particles one from another, and to their reunion. No new creation or destruction of matter is within

* Henry's Life of Dalton, p. 51.

the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce consist in separating particles that are in a state of cohesion or combination and joining those that were previously at a distance."

The idea of chemical combination as a union of particles of determinate mass once conceived, soon found support in other directions. Dalton himself gave examples, including those already quoted, of the law of Multiples.

The law of Definite Proportions was established by Proust in 1808 as the result of a long experimental enquiry, and the law of Reciprocal Proportions Dalton himself recognised in the work published many years before by Richter. The results of all these researches were accordant with Dalton's views, and gave his theory the solidity which it would otherwise have wanted. Subsequently, a very powerful support of the Atomic Doctrine was supplied by Gay Lussac's Law of Volumes as applied to gases; though, strange to say, Dalton himself failed to perceive its importance, although recognised by all his contemporaries.

Dalton's conceptions regarding the form and dimensions and nature of his atoms were of a simple mechanical kind, and, if we may judge by the diagrams which he made use of to illustrate combination, he supposed, like Newton, that matter was formed of solid, massy, hard, impenetrable, movable particles; "and that those primitive particles join together under the influence of chemical affinity, and separate under the influence of heat."

According to Dalton (Chem. Phil., Part I., 147), "A vessel full of any pure elastic fluid presents to the imagination a picture like one full of small shot. The globules are all of the same size, but the particles of the fluid differ from those of the shot in that they are constituted of an exceedingly small central atom of solid matter which is surrounded by an atmosphere of heat," and so forth.

At this point it may be as well to recall the fact that Dalton and several of the early promoters of the atomic system were led to consider, though without giving the subject much attention, the question of the arrangement which united atoms occupy in space. Thus Dalton (Chem. Phil., Part I., 216), in referring to the diagrams, says "the combinations consist in the juxtaposition of two or more of these (atoms); when three or more particles of elastic fluids are combined together in one it is to be supposed that the particles of the same kind repel each other, and, therefore, take their stations accordingly." And Wollaston, in a paper in the Phil. Trans., 1808, remarks "that we shall be obliged to acquire a geometrical conception of their relative arrangements in all the three dimensions of solid extension."

Some forty years elapsed before the germ of the more modern doctrine of valency was recognised. Liebig and Wöhler discovered benzoyl, Bunsen discovered cacodyl, and Frankland obtained ethyl and methyl, and studied their compounds, and so compound radicles came to play an important part in the theory of the constitution of carbon compounds. Then came Gerhardt's system of types, and out of this grew directly the idea that certain atoms or radicles have the power of holding together in one group or molecule two or more other atoms or radicles, and so it came to be known that atoms are not all equal in their capacity of entering into chemical combination.

In considering this question of valency, as in that of the atoms, it is very important to clearly distinguish fact from hypothesis.

It is an observed fact that the several elements, in the proportion of one atom of each, habitually associate with one, with two, with three, with four, or more atoms of another kind; and that, with comparatively few exceptions, elements which have a capacity of combination represented by an even number of other atoms do not enter into combination with an uneven number of other atoms; while those which habitually

combine with one, three, or five other atoms, rarely unite with two, four, or six.

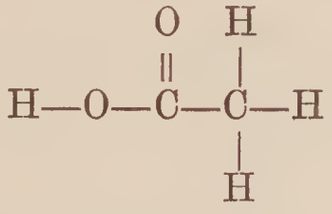
These are facts; but what do they point to? Is valency due to

(*a*) A state of motion peculiar to each kind of atom; or,

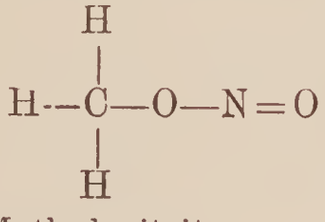
(*b*) to the configuration of the atom; or,

(*c*) to the possession of special poles or points of attraction? etc.

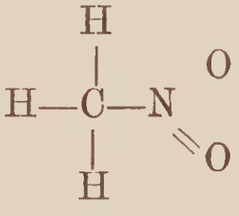
Until some eighteen years ago, ideas of constitution based upon valency had led only to the adoption of a system of formulæ which, though they were called constitutional, had no pretensions to representing the actual arrangement of atoms in space. Constitutional formulæ have generally been used much as Gerhardt's type formulæ were always used, merely as affording a convenient means of epitomising the chief reactions of bodies. Thus, if acetic acid was expressed as



this formula was not designed to serve as a picture of a molecule, but merely to recall certain facts in the chemical deportment of the compound. This kind of formula has also been very serviceable for the purpose of distinguishing the more prominent cases of isomerism. Thus



Methyl nitrite



Nitromethane.

Two lactic acids, having the formulæ $CH_3.CHOH.COOH$, are known, and these are very much alike, as are also in general their corresponding salts; but one of the acids is optically active—that is, it causes the rotation of a plane polarised ray—while the other is inactive. Ordinary constitutional formulæ,

written upon a plane surface, are incapable of expressing a difference in cases of this kind. The study of the lactic acids seems to have led Wislicenus, so early as 1872-3, to conceive the idea that the differences observed might be attributable to different relative positions of the atoms in space of three dimensions. The most systematic and complete application of this principle, however, is embodied in the views of Van t'Hoff, which were set forth in his treatise, "La Chimie dans l'Espace," published in 1875. The hypothesis has been applied at present almost exclusively to compounds of carbon, and attention has been concentrated chiefly upon the atom of carbon itself, without much reference to the questions presented by the other elements associated with it.

Van t'Hoff's hypothesis is based upon the following admitted facts :—

1. The four units of valency of carbon are equal in every respect. In the mono-substitution derivatives of marsh gas and ethane no isomeric modifications have been discovered.
2. All compounds of carbon which in the liquid state rotate a polarised ray, or when crystallised produce enantiomorphous hemihedral forms, are found to contain at least one atom of carbon, which is united directly to four dissimilar atoms or groups of atoms, and which is therefore said to be asymmetric.
3. Compounds which are known to contain asymmetric carbon, and which, nevertheless, do not exhibit optical activity, are generally resolvable by one or other of several known processes* into two

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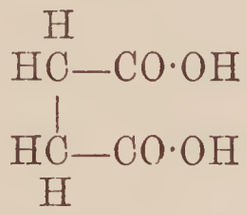
* The several methods hitherto employed for the resolution of optically inactive mixtures of bodies containing asymmetric isomerides are as follows :—

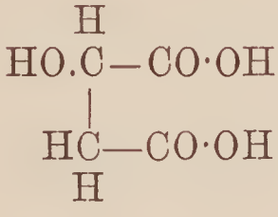
1.—Mechanical separation of enantiomorphous crystals as in the case of racemic acid, originally investigated by Pasteur. The ammonium sodium racemate yields two hemihedral forms, of which one is the reflection of the other. When these are picked out and separated, the acids obtained from them have equal opposite rotatory power.

compounds, each of which possesses rotatory power equal and opposite in direction to the rotatory power of the other.

Succinic acid, for example,



is optically inactive; but when one of the hydrogen atoms is replaced by hydroxyl, so that the C to which it is attached becomes "asymmetric," the result is the production of malic acid,



which exists in two isomeric forms, one of which rotates to the right, the other to the left (Bremer Ber., xiii. 351*a*). Some exceptions seem to exist in the case of compounds which contain

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A convenient modification of this process consists in preparing a supersaturated solution of the ammonium sodium racemate, and then introducing as a nucleus a crystal of the desired modification. Crystallisation then occurs, and the opposite modification is left in the mother liquid.

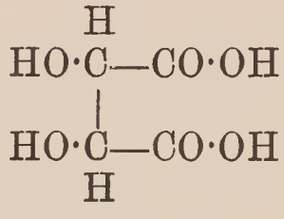
2.—The growth of organisms of the nature of moulds in suitable solutions generally leads to the destruction of one of the active varieties before the other. Thus penicillium glaucum grown upon racemic acid uses up the dextro-acid first. Consequently, by interrupting the process at the proper stage, the lævo-acid may be obtained pure. Yeast, also, introduced into a solution of inverted sugar ferments the dextrose more rapidly than the lævulose.

3.—When a mixture of two oppositely active substances is added to another substance, with which they can combine, containing asymmetric carbon, a preferential union usually occurs with one or other of the constituents of the mixture. Thus inactive malic acid was separated into two active varieties by mixing it with successive doses of cinchonine, which forms a compound with the dextro-rotating acid which crystallises first, leaving the lævo-acid in the solution.

On the other hand, the constituents of an inactive mixture of active bases may be separated by the aid of an optically active acid. Ladenburg, in this way, analysed coniine.

an atom of carbon, but are of very simple constitution; for example, bromo-nitroethane, $CHBr.NO_2.CH_3$, which is inactive. This, however, may arise from the strongly negative character of the halogen atom, as it has been found generally that compounds having a halogen attached to the asymmetric atom are optically inactive (Easterfield, J. Chem. Soc., Jan. 1891, p. 71.)

Another kind of apparent exception is represented by mesotartaric acid. This compound has the same composition as dextro-tartaric acid, lævo-tartaric acid, and racemic acid, which is resolvable into a mixture of the two former. Mesotartaric acid is not so resolvable, and therefore cannot be regarded, like racemic acid, as a compound of the other two. But it contains within the molecule two asymmetric carbon atoms—



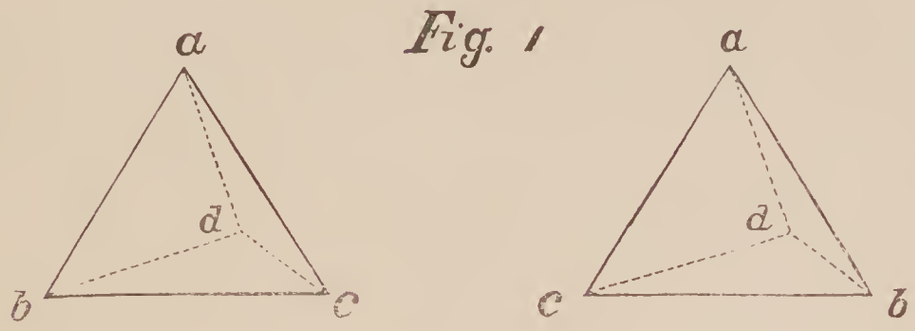
and the action of one of these on the polarised ray may be supposed to be equal and opposite to the action of the other, so that the effect is the same as if they existed in separate molecules, mixed together in exactly equal numbers, as in the case of racemic acid.

Van t'Hoff's hypothesis, which serves to explain these facts, supposes the carbon atom to be situated at the centre of a regular tetrahedron, while the four other atoms united with it are situated at the solid angles; so that the four valencies of the carbon atom are supposed to operate in the directions of four radii of a sphere included in the tetrahedron, or which includes it.

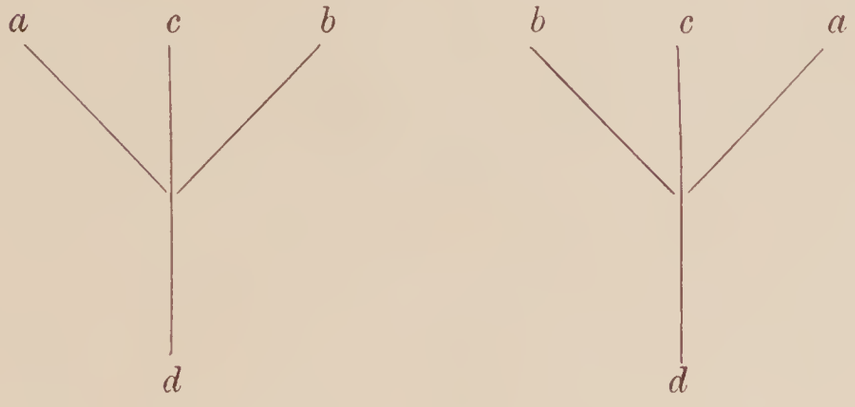
Suppose the atoms united with a carbon atom to be represented by letters, then when one atom of carbon is united with 4A, with 3A and 1B, with 2A and 2B, or with 2A, 1B, and 1C, isomerism is impossible; but when all four are different, A, B, C, D, two cases are possible. These may be represented

either by a figure of a tetrahedron, the angles of which are lettered as in Figure 1 ;

Fig. 1



or by a conventional symbol, which is more easily printed thus—



One of these is obviously a reflection of the other, and is not superposible upon it in such a way that the letters coincide.

The use of models assists materially in the consideration of the problems arising out of this hypothesis. One of the first questions which arise relates to the direction in which the several valencies of an atom of carbon may be supposed to be exerted. If the direction of these be supposed to be absolutely fixed, then it can be shown that—

1. Two carbon atoms cannot unite by two bonds, nor by three, because that would involve the distortion of the atom.
2. Three or more carbon atoms cannot unite to form a ring for the same reason.

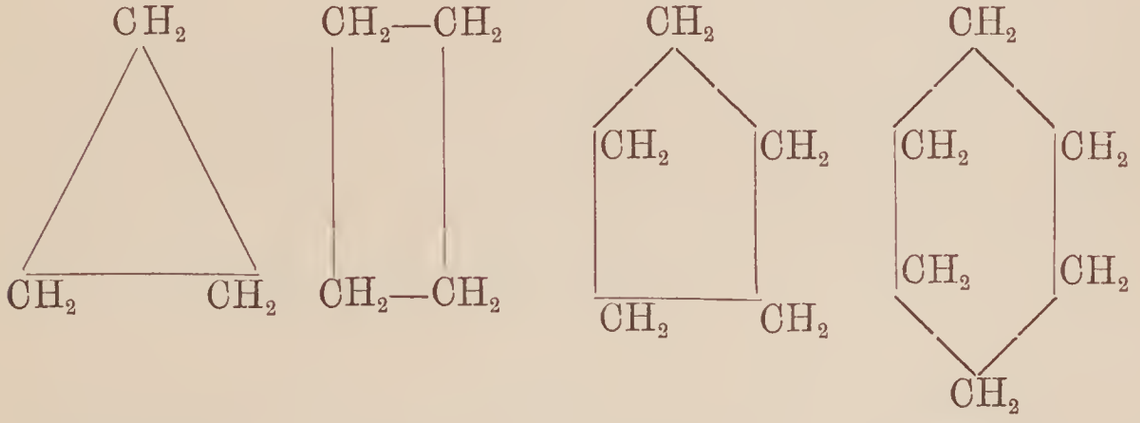
Hence it seems that if the direction of the attractions of carbon for other atoms can be determined at all, the line of attraction must be rather easily displaceable from the normal, or it operates somewhat like the pole of a magnet, that is, there is a certain *field*. The analogy between the attractions of two atoms for each other, and the attraction exercised between the opposite poles of two magnets, or between a magnet and a piece of iron, may in fact be considered as extending even further. For just as the presence of a mass of iron in the neighbourhood of a magnetic pole seems to absorb the lines of magnetic induction and so reduce the action of the magnet upon other bodies placed near, so the addition of one element to another in chemical union diminishes the tendency of either to combine with a third element, but does not, in the majority of cases, absolutely extinguish this tendency. Hence we have what has been called "residual valency," which, whatever may be the fate of the various hypotheses concerning it, gives rise to very well marked phenomena of combination. It is, however, usually believed that two carbon atoms may actually be united by two or more units of valency, but that in all such cases the combination it not only not more secure, but is decidedly more easily broken up than where one bond only of each is employed. To account for this difference, so contrary to what might be at first sight expected, two chief hypotheses have been proposed. The first, which originated with von Baeyer,* is based upon the same hypothesis with regard to the carbon atom as that of Le Bel and Van t'Hoff. If the valencies of the carbon atom act along the lines drawn from the centre to the solid angles of the regular tetrahedron, these lines form with each other angles of 109° 28′. If two carbon atoms are so situated towards each other that two valencies of each are united together so that the directions of the two are parallel to each other, each is turned out of its normal position by an angle of 54° 44′; and if two carbon atoms are similarly joined by three units of valency,

* Ber. XVIII. 2277.

each of these must deviate 70° 32′ from its normal position. It is known that combination by double or triple bonds is easily converted into union by single bonds in all cases of this kind. Such substances as acetylene and ethylene are saturated with great readiness, not only by bromine, but by hydracids, and even by iodine, and the tension is thus relieved.

CH_2	CH_2H	CH_2Br
‖	|	|
CH_2	CH_2Br	CH_2Br
Ethylene.	Ethyl bromide.	Ethylene bromide, &c.

V. Baeyer has also pointed out that in the formation of rings of carbon atoms the distortion of the atoms diminishes as the number of carbon atoms increases up to five. In a ring of six carbon atoms united by single bonds the distortion is a little greater. The following angles represent the extent to which the direction of each valency is disturbed.



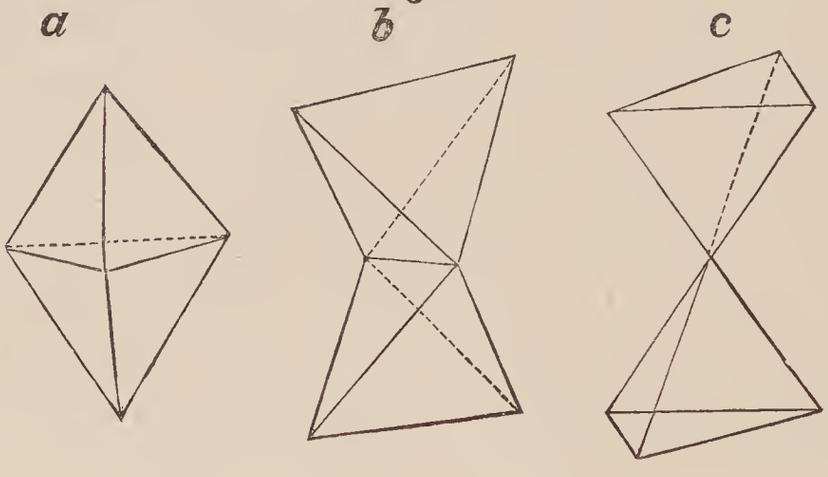
24° 44′ 9° 34′ 0° 44 + 5° 16′

The other hypothesis, proposed by Wunderlich,* is based upon the idea that the relative force of attraction between two units of valency depends upon the relative distances through which they have to act. Briefly, the hypothesis is somewhat as follows. The carbon atom is a sphere from which four equal

* Configuration Organischer Moleküle, Würtzburg, 1886. Abstract in Ber. XIX., 592c.

symmetrically-placed segments have been removed, and the circular faces thus formed are situated in the planes of the four faces of a regular tetrahedron. When combination takes place between two carbon atoms the most intimate union is that in which two of these faces are placed parallel to, and probably very near, each other. A less intimate union occurs when the centres of gravity of two faces of one atom attracts two faces of another. The two tetrahedra have then a common edge, the two pairs of faces forming equal angles with each other. And, lastly, three faces of one may attract equally three faces of the other, and so cause the two tetrahedra to be applied to each other by one of their solid angles. These three positions correspond to combination by single, double, and triple bonds.

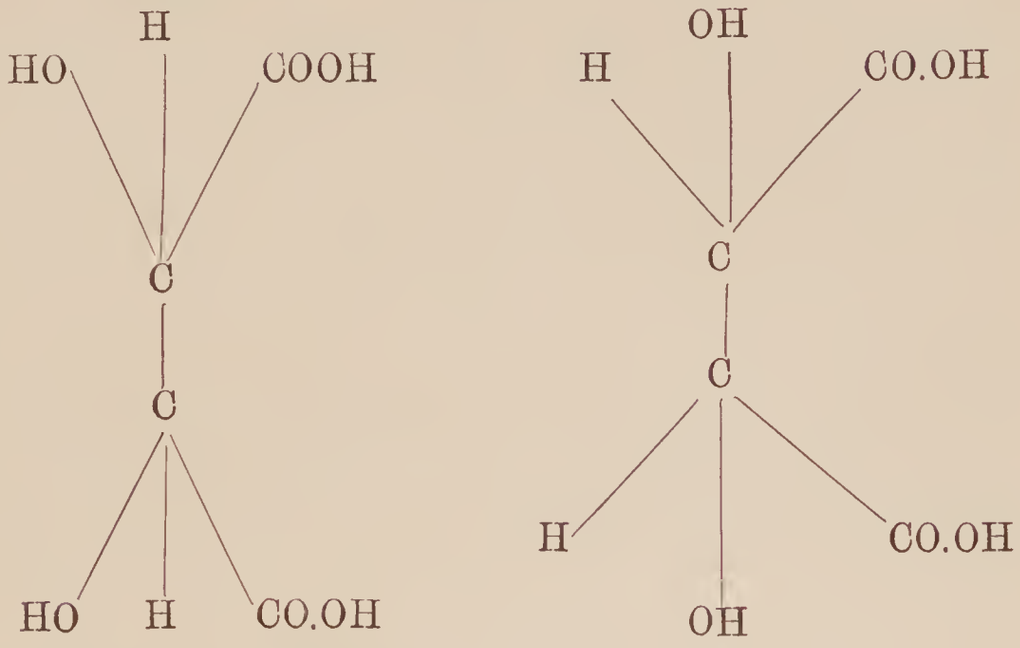
Fig 2



According to this assumption it is only possible for the atoms to touch each other when united by the single bond, as shown at *a*.

When two asymmetric carbon atoms are united together the number of possible isomeric forms is greater. Tartaric acid is a case of this kind. Four isomeric acids of the same composition are known, namely: dextro-tartaric acid, lævo-tartaric, meso-tartaric, and racemic acids. Racemic acid is known by the researches of Pasteur, made forty years ago, to consist of a mixture or molecular compound of dextro- and lævo-tartaric acids.

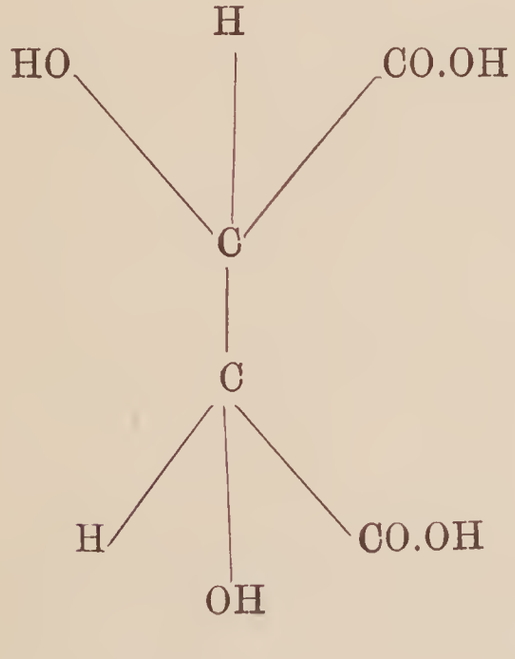
These two latter are supposed to be related to each other in the manner indicated in the following formulæ :—



which may be more briefly written :

$$\begin{matrix} r \\ | \\ r \end{matrix} \quad \text{and} \quad \begin{matrix} l \\ | \\ l \end{matrix}$$

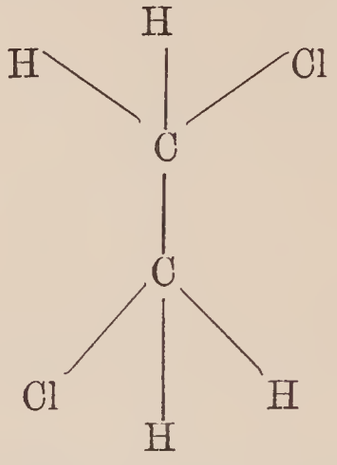
where the letters r and l represent an arrangement which results in right or left-handed rotation of the polarised ray. Meso-tartaric acid, which, as already explained, is optically inactive, though containing two asymmetric carbons, is by extension of the same hypothesis represented as



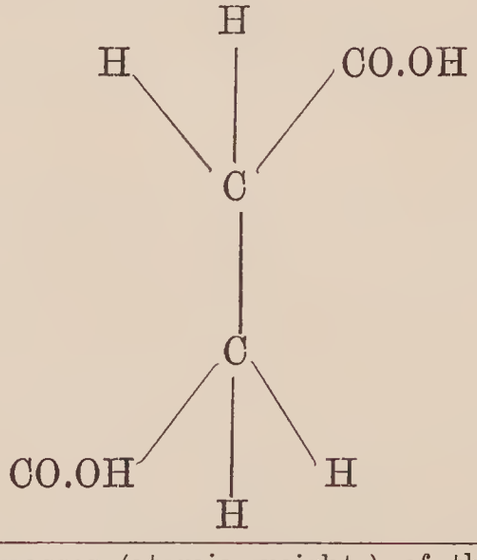
or $\begin{matrix} r \\ | \\ l \end{matrix}$

Van t'Hoff long ago indicated that when two carbon atoms are united together by a single bond they may be supposed to be free to rotate about an axis which is in the line representing the direction of the uniting valencies, and that if two carbons are joined by two or more bonds rotation becomes impossible. This hypothesis was first made use of by Wislicenus in 1886, and has been the subject of a good deal of discussion since.

It may be assumed that the radicles united to two adjacent atoms of carbon will be likely to influence each other, * and, according as they attract or repel each other, rotation may or may not occur. Thus it may be supposed that in Dutch liquid the chlorine and hydrogen atoms probably attract each other. Hence there can be only one stable form of this compound, viz. :—



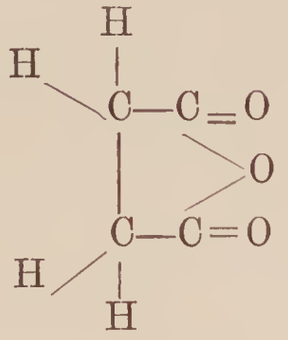
other arrangements passing spontaneously by rotation into this.† Succinic acid also is known in only one form, which probably has the following structure :



* The relative masses (atomic weights) of the atoms or atomic groups may have something to do with this.

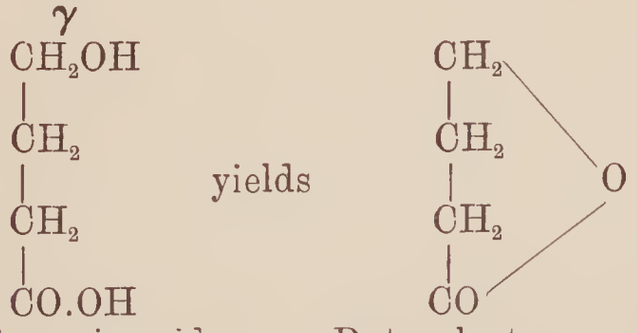
† That atoms which are not directly united do influence each other is certain from such cases as the O of the carboxyl group, the characters of bromnitroethane, the chloranilines, &c.

If this be so, it seems to follow that rotation must occur when the anhydride is found under the influence of heat :—



From other similar cases it appears probable that the formation of rings, in which four carbon atoms, or four carbon atoms and one oxygen atom are concerned, probably occurs in one plane. And von Baeyer has pointed out that the interior angles of a regular pentagon are very nearly equal to the angle which the valencies of carbon in their normal position form with one another. This appears to explain such facts as the ready production of anhydrides from dibasic acids, such as succinic acid, already referred to, and phthalic acid, &c.; also the formation of lactones by loss of elements of water by the γ hydroxy-acids of the fatty series, such as oxybutyric acid. It is certainly interesting to compare the action of heat upon a β acid with that of a γ acid of the same series ; for example :

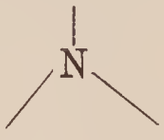
Hydracrylic acid — Acrylic acid



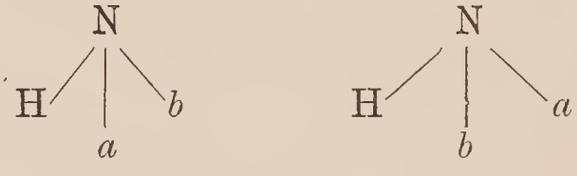
Oxybutyric acid — Butyrolactone

The hypotheses, of which an outline has been given, relate to the atom of the element of carbon. Considerations of this kind naturally lead to the enquiry whether it is possible to apply the same fundamental idea to the study of other multivalent elements, and the case of nitrogen presents itself first.

It appears to be generally agreed that the three valencies of nitrogen in ammonia and its immediate derivatives are disposed symmetrically, but a representation of this fact requires that their directions shall be represented either on one plane



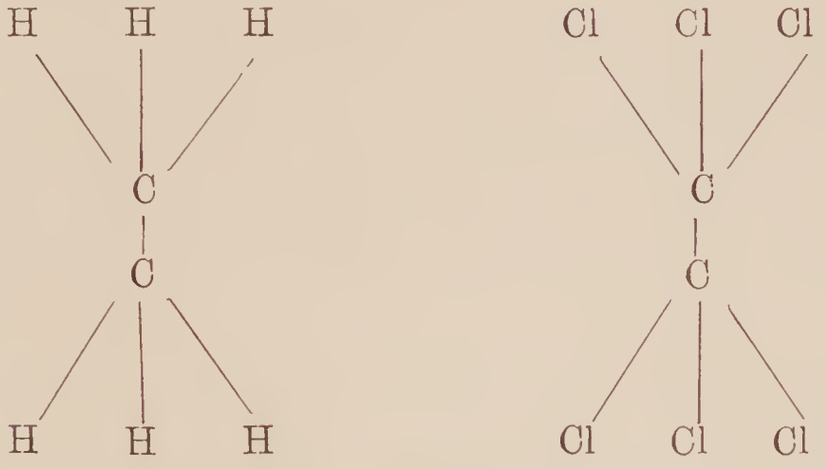
or the centre of mass of the atom may be supposed to occupy one solid angle of a regular tetrahedron, the direction in which the valencies act being represented by the edges of the figure. If the tetrahedral form of ammonia be assumed, there should be two isomeric forms of derivatives, in which *a* and *b* represent the replacing radicles,



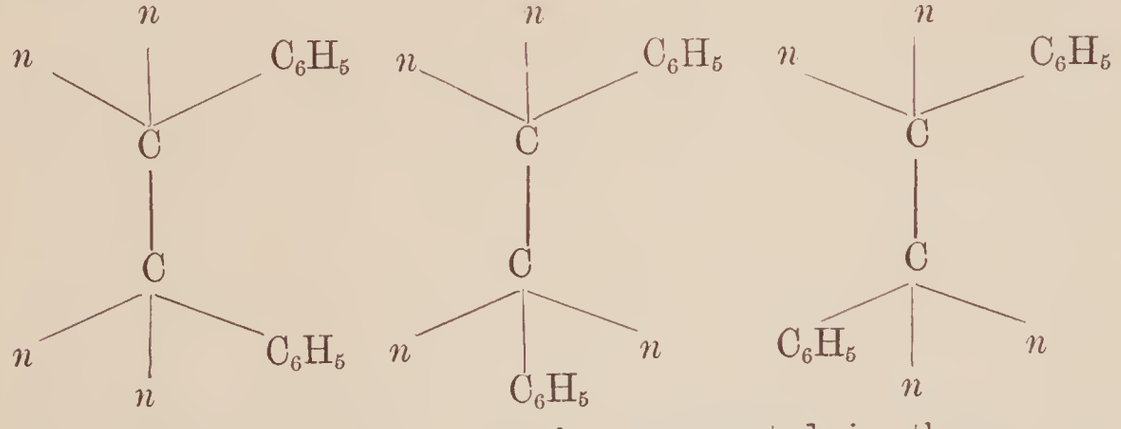
the one being a reflection of the other. Such compounds as these, however, have never been observed, either among the methyl or acetyl derivatives of ammonia, or in the case of hydroxylamine, of which no physical isomerides are known.

The aldehyds, ketones, and ketonic acids yield, by the action of hydroxylamine, a large number of compounds called oxims or hydroximides, which are generally believed to contain the residue of hydroxylamine, $=N.OH$, united to an atom of carbon in exchange for an atom of oxygen. Similar compounds are formed by the action of nitrous acid upon compounds containing $=CH_2$. Thus benzil $C_6H_5.CO.CO.C_6H_5$ is capable of giving either a monoxim $C_6H_5.C(N.OH).CO.C_6H_5$, or a dioxim $C_6H_5.C(N.OH).C(N.OH).C_6H_5$. In the course of an investigation of these compounds, Victor Meyer observed, in 1888, that there

were two isomeric forms of the dioxim the existence of which could not be explained by current hypotheses. A third dioxim of benzil has since been obtained. It seems probable that the free rotation of two singly-bound carbon atoms is restricted when there is a certain relation or opposition among the attached radicles. Hence, while there is but one known ethane or hexchlorethane,



the existence of three and even four varieties of the benzil dioxim, capable of isolation and not passing spontaneously into one another, may be explained by the use of this hypothesis. Using *n.n* to represent the two valencies occupied by the N.OH group, the formulæ of the benzil dioxims may be represented thus:—



The "stereo-isomerism" can be represented in these cases without special assumptions with regard to the nitrogen. Hantzsch and Werner, however, have adopted * the hypothesis of the tetrahedral configuration of the nitrogen atom. According to this view, the residue =N.OH may be attached

* A summary of their views is given in the Berichte 23, 2764-2769.

to a carbon atom united with two radicles in such a way that when the two radicles are of the same kind, say X, there can be but one product; whereas, when the carbon is united to two different radicles, X and Y, there may be two stereo-isomerides.

$$\begin{array}{c} X\cdot C\cdot Y \\ \| \\ HO\cdot N \end{array} \qquad \text{and} \qquad \begin{array}{c} X\cdot C\cdot Y \\ \| \\ N\cdot OH \end{array}$$

The existence of isomeric forms of this kind is, however, a question which still requires investigation.

Up to the present time experimental enquiry on the subject of stereo-isomerism has been limited to compounds of carbon and nitrogen. I am not aware of any publications in which the possible configuration of the atoms of other elements is discussed. Silicon, for example, is an element which in many of its relations resembles carbon. Silicon forms a tetrachloride, a tetrahydride, a tetrethide, and an ethyl orthosilicate. An atom of silicon can also replace an atom of carbon in such compounds as silicopropionic acid $C_2H_5.Si\,O\cdot OH$. Hence it is probable that the silicon atom, like that of carbon, might be represented by a regular tetrahedron. The atomic weight of silicon being, however, 28, while that of carbon is 12, the dimensions of the silicon tetrahedron would certainly be different from those of the carbon tetrahedron; and, to account for the differences in physical and chemical properties between silicon and carbon compounds, it is obvious that there is plenty of room for conjecture.

Similarly we may suppose that whatever hypothesis is ultimately adopted in respect to nitrogen, something on the same pattern would apply to the cases of phosphorus and arsenic.

The difficulties which surround the whole subject are, however, very great, even when enquiry is restricted to the case of elements of acknowledged similarity. Examples, however, are known of elements of one type simulating the behaviour of elements of a totally different type, as when sulphur in the thetines seems to represent nitrogen in all but valency, and when arsenic replaces sulphur in many minerals.

All that is known seems to show, that however useful the hypothesis of Van t'Hoff may be at the present stage, it can only be regarded as a working hypothesis which, after a time, must be discarded in favour of some more comprehensive theory, which, perhaps, may represent one step nearer to the physical truth.

I have not referred to the electrical theories of chemical combination and decomposition, because our knowledge of electrolytic phenomena is not sufficient at present to enable us to come to definite conclusions, and a vigorous discussion of the subject is still in progress; and lastly, because an explanation of the cause of valency did not seem to be necessarily involved in the hypotheses of which an account has just been given.

The theory of vortices has found favour among physicists, and is in many ways attractive to the chemist. Nevertheless, so far as I can understand it, it seems to involve some conclusions that are not borne out by experience of chemical phenomena, for example, that the valency of no elemental atom can be greater than six units, and that the number of links in the atoms of carbon is only twice as many as in the atom of hydrogen, and so forth. It also seems to preclude the idea of what is often called "residual valency," or the surplus capacity for combination exhibited more especially by the halogens and by oxygen and sulphur after their normal valency has been satisfied by combination with other atoms.* Van t'Hoff's hypothesis and the extensions of it, which have been referred to in the foregoing pages, possess, on the other hand, this recommendation, that they have not only served to connect a certain optical property with a peculiarity of chemical constitution, but the application of the theory has led to the discovery of a number of isomeric compounds, the existence of which had not previously been suspected. The employment of a hypothesis which leads to discovery may be said to be something more than excusable; it is necessary.

* See Motion of Vortex Rings, by J. J. Thomson, p. 120, *et seq.* Macmillan, 1883.

VIII.—*The Cause of Imperfections in the Colour of Sheet Brass.*

By Thomas Turner, Assoc.R.S.M., F.C.S., F.I.C.,
Lecturer on Metallurgy at Mason College.

[Read before the Society, May 6, 1891.]

During the last few years my attention has frequently been directed to a particular kind of copper-coloured or brown stain or marking which not uncommonly occurs on the surface of finished sheet brass. Many sheets are quite free from this imperfection, but in others the stains cover a considerable proportion of the surface, being sometimes in the form of distinct spots or rings, but usually in bands or belts such as one might suppose could be produced by the rolling out of a small globule of copper or rich copper alloy embedded in a matrix of yellow brass.

These imperfections, of which examples are shown, are met with in brasses of varying composition, and are common in button metal, cartridge metal, and other forms of good sheet brass, and their presence frequently involves considerable loss and trouble. Thus in the case of button metal the blanks have to be sorted to separate those which are discoloured, and which are therefore unsuited either for use as bright brass, or for bronzing or tinning. The discoloured blanks are of inferior value, and are usually japanned and sold as black buttons. In many other branches of the Birmingham sheet metal trades these discolourations are equally troublesome.

A number of explanations have been suggested in order to account for the production of these copper-coloured stains, among the chief being the following :—

1.—That they were caused by inattention on the part of the workman in allowing the sheets to become overheated during annealing, by which it is alleged that zinc is volatilized from the surface of the sheet, and a coating of copper produced. To this it may be objected that if such an action took place there is no reason why the copper film should be produced in spots or bands instead of a uniform film, as might be expected if the whole sheet were overheated. But, in addition, it is easy to experimentally prove that overheating does not produce this effect ; for, if a sheet of brass be heated in a muffle until a portion actually melts, it will still be found, on removing the scale from the remaining portion by pickling, that a fairly uniform colour is obtained.

2.—The workmen themselves assert that these stains are due to sulphur in the coal. Of course "sulphur" as used by the workmen includes combustible gases and the products of combustion, and does not necessarily mean the element sulphur at all. But, as in the previous case, it is not easy to see why if the whole of the sheet is exposed to the same gases these gases should produce a deep colour on one portion of the surface and no colour at all on the parts immediately adjacent.

In order to test this matter I heated ordinary sheet brass in a gas muffle to a full red heat, and from time to time introduced fragments of sulphur so as to keep the atmosphere charged with sulphur dioxide. The result is shown in Sample 1, the surface of which, after having been cleaned in dilute sulphuric acid, is rather rough and dull, but practically free from the stains referred to. To further test this point, Samples 2 and 3 were prepared. On Sample 2 flowers of sulphur were sprinkled in the form of a letter S, and the metal was then heated as before. After pickling, the position of the sulphur is only indistinctly shown on the surface by dark coloured stains. On Sample 3 a heap of sulphur was placed, but after heating

and subsequently pickling, the result obtained was a dark coloured stain with light coloured spots. Obviously, therefore, sulphur or sulphurous gases cannot be responsible for the production of the red stains previously mentioned. But it appeared possible that coal dust, or coal ashes, or soot, or a reducing atmosphere might assist in the reduction of zinc oxide, which is non-volatile, and so assist in liberating zinc from parts of the brass and produce the objectionable red colouration.

I, therefore, tested a number of samples by covering a portion of each of them with coal dust, coal ashes, charcoal powder damped with a solution of gum, chalk, &c. In each case the result was very similar, and is illustrated by Sample 4, on which coal ashes were dusted in the form of a letter A, and after pickling, the pattern is observed to remain as a faint and not very definite dark stain. It appears to me, therefore, that the imperfections we are considering cannot be due to variations in any of the furnace conditions above enumerated.

3.—Perhaps the most natural suggestion, from the general appearance of the imperfect sheets, is that which has met with considerable favour in some quarters, namely, that the brass is not uniform in composition, but that owing either to imperfect mixture or to separation during cooling, the constituent metals have, more or less, separated in places, and that the red colouration is due to the greater concentration of copper in certain parts. It must be remembered, however, on the other hand, that all trustworthy experiments on this subject have shown the very remarkable uniformity of composition which exists in copper zinc alloys.

Dr. Tilden allows me to mention that, in connection with his experiments on the Corrosion and Pitting of Copper and Brass by Saline Waters (Jour. Soc. Chem. Indust., Feb., 1886), he paid special attention to the question of the uniformity in composition of the sheet metal employed, and, as the result of a large number of tests, he was unable to find any evidence to support the view that separation of richer copper alloys occurred.

The researches of Laurie (Jour. Chem. Soc., 1888, p. 88) also give strong support to Dr. Tilden's conclusions. In one sample of doubtful origin I believe I have obtained distinct evidence by etching with nitric acid that such a separation had taken place. though probably the specimen was from the "spilley" end of the strip, as the little grains of red alloy were distributed in a manner such as might be expected under the circumstances. But by etching a large number of other samples of sheet brass with varying proportions of copper, I have never met with another instance of the same thing, and any such separation must be very unusual.

In order to test this third explanation, I have selected a number of strips of sheet brass which were very plainly marked with bright copper-red spots and bands, the markings being on both sides of the sheet, and giving the appearance of nodules of a separated alloy passing through the thickness of the sheet. These strips were etched by immersing in diluted nitric acid for about twelve hours, but the resulting strip was in each case quite uniformly crystalline in texture after the outer surface had been removed, and no indication of variation in composition was obtained. Samples 5 and 6 illustrate these experiments, the part which was etched and now shows a perfectly uniform crystalline character, being originally very plainly marked with copper-red discolourations. Further proof that only the surface is affected by these discolourations is afforded by the fact that the copper can be scraped off with a knife or a file, when ordinary yellow brass is found underneath ; while, on the other hand, so far as I am aware, these red markings are not laid bare when, in the process of working, the surface is removed from a piece of ordinary bright brass. It appears evident, therefore, that the imperfection of surface cannot be due to irregular distribution of the constituent metals of the alloy, and I have previously shown that it is not caused either by overheating or by furnace gases, &c., during annealing.

Attention was thus directed to the method whereby the sheets are cleansed from scale between each annealing. This is

usually accomplished by dipping the sheets in diluted sulphuric acid, and subsequently washing with water. The washing is often doubtless far from perfect, and the wash water is generally impure, so that it is not unreasonable to suppose that sheets are frequently subjected to a second annealing when their surfaces are by no means chemically clean. In order to test these points the following experiments were made :—A small sheet of brass was prepared, and the corners slightly turned up, so as to make a very shallow evaporating pan ; a small quantity of distilled water was evaporated to dryness on this pan, and it was then heated to redness in a muffle and afterwards cleaned with dilute sulphuric acid. The surface was found to be marbled all over with faint reddish-brown stains (Sample 7), doubtless due to the action of the minute quantity of solid residue left by ordinary distilled water. Birmingham tap water, which is moderately hard, when tested in the same manner (Samples 8 and 9) gave very distinct red discolourations, though these were not so plain as those commonly observed in commercial samples of sheet brass.

Dilute sulphuric acid, containing 1 per cent. of commercially pure acid, gave a very marked dull copper-brown stain (Sample 10), darker than that obtained in practice, and this stain was quite visible with water containing so little sulphuric acid as 1 part in 500 (Sample 11). A solution of sodium chloride, containing 1 per cent. of Na Cl gave a very marked bright copper-red stain (Sample 12), which is still very visible with 1 part of salt in 1,000 of water (Sample 13).

Solutions of calcium, magnesium, and potassium chlorides produce a similar effect. Galvanizers' waste pickle (a solution of ferrous chloride, not unknown in local canal water) gives a similar bright copper stain even when diluted with 500 parts of water (Sample 14). Ordinary ink, as might be expected, produces a bright red discolouration (Sample 15). Copper sulphate produces a faint light-coloured stain (Sample 16), copper nitrate a copper-red (Sample 17), while zinc chloride gives scarcely any copper colour at all (Sample 18).

This apparently indicates that the chlorides, or other salts of metals other than zinc, are decomposed at a high temperature, and the liberated acid attacks the zinc in preference to the copper, which is thus left, and causes the red stain on the surface of the sheet.

In the foregoing experiments, though the cause of the colouration was discovered, no really quantitative values were obtained as to the amount of the substances necessary to produce a distinct effect, since the liquid became concentrated during evaporation, and the colour observed was due to the concentrated liquid thus obtained. To obviate this difficulty a few more samples were prepared, in which the liquids to be tested were used as writing fluids, and applied with an ordinary pen, for the purpose of recording the substance used and strength of solution. By this means a thin film of liquid of a definite shape was obtained, and the effect produced could be easily observed. Thus Sample 19 is marked with a faint dull brown stain "H_2SO_4 1 per 100," while in Sample 20 the "H_2SO_4 1 per 500" is incomplete, and is very faint where it can be seen. Hence the limit of ill effect of sulphuric acid is reached with 1 of acid to 500 of water.

On Sample 21 can be plainly seen in bright copper "Salt 1 per 100," while on Sample 22 "SALT 1 per 1000" is still distinctly visible, showing that salt not merely produces a brighter stain, but that an exceedingly small quantity is required to produce a discolouration. In fact, this reaction is so delicate that it might be used as a qualitative test for the presence of chlorides in water. On Sample 23 can be read in bright letters "Waste Pickle 1 in 50," while Sample 24 shows that "Waste Pickle 1 in 100" produces a very marked discolouration, and this probably corresponds to about 1 part of chlorine per 1000 of water. When a thin film of liquid produces so distinct a stain with only 1 part of chlorine in 1000 of water, it is evident that by using a few drops of the water to be tested 1 grain of chlorine per gallon might be readily detected.

From these facts I concluded that the use of impure water

for washing the sheets after pickling was the most probable cause of much of the discolouration noticed in sheet brass. On enquiry as to the practice at certain works in the neighbourhood this view was amply confirmed, for I am informed that at one of the rolling mills from which I have received a number of samples of discoloured metal, after pickling they are in the habit of washing the sheets in canal water, and not unfrequently the metal is allowed to dry spontaneously by the action of the atmosphere, thus depositing a thin saline crust on the sheets. When it is added that a galvanising works is situated on the same canal, in the vicinity of the rolling mill, no further explanation is needed as to the cause of the red spots or bands on their finished brass. In such a case the remedy is simple. Care should be taken to wash away all the sulphuric acid pickle used in cleansing the sheets, and the last washing water should be as pure as possible, while the presence of chlorides in any form should be especially avoided. Probably, also, when water of only moderate purity can be obtained, some method of drying the sheets instead of spontaneous evaporation would be a considerable improvement.

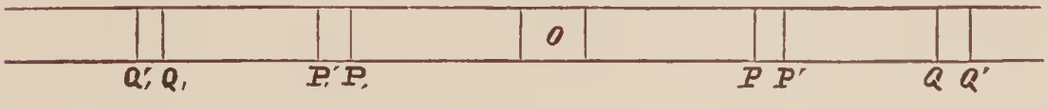
IX.—*Observations on the Law of Facility of Errors.*

By the Rev. H. W. Watson, D.Sc., F.R.S.

(Read before the Society, February 11, 1891.)

One of the most interesting questions in the science of probabilities applied to statistics is whether there is such a thing as a law of error—or rather, a law of facility of errors—a question, in fact, which may be stated thus :—

Fig. 1.



AB is a thin strip of cardboard of uniform width, infinite, or practically infinite, in length, used as a linear target.

At O is a small strip bounded by very near partitions serving as a bull's-eye.

A marksman aims at the bull's-eye from some standing point. Can we, with any certainty, accept any law or formula

which shall express the reasonable probability that his shot shall hit the target at any given distance from O, or, to be more precise, the chance that it shall hit the target between two distances very close to each other?

For example: suppose the target divided into sections each one-tenth of an inch long, as PP′, QQ′, &c., on the right of O, and P_1P_1', Q_1Q_1', on the left; and suppose OP and OP_1 to be each m inches, and OQ and OQ_1 to be each n inches, is there any formula or algebraical expression in terms of m and n which will give the chances of the shot being in any section?

Now I may state here that no such law of facility has been proved by any irrefragable demonstration, at least so far as my own knowledge extends, but that there is a law or formula pointed at by many independent investigations, and, I think, very generally accepted in the reduction of observations. I have myself very little doubt that the law is really true, at least if used with proper precautions. That law is that if the distance from O, the centre of the bull's-eye, to the centre of any small section to the right of O, as PP′, be called x, or to the left of O, as P_1P_1', be called $-x$, and the length of the section be (dx), then, as (dx) is indefinitely diminished, the chance of the shot falling in that section is

$$\frac{1}{a\sqrt{\pi}} e^{\frac{-(h-x)^2}{a^2}} dx$$

where h and a are certain small quantities diminishing as the skill of the marksman increases, and depending upon that skill in each particular case.

The quantity h depends upon the right or left hand bias, if any, on the part of the marksman, and if such bias did not exist h would be zero, and the law would be expressed by the simpler form

$$\frac{1}{a\sqrt{\pi}} e^{\frac{-x}{a^2}} dx$$

For simplicity's sake, and as no principle is involved in this assumption, I shall make it, and in referring to the law of error assume that law to be as last written.

The quantity a, which is the square root of the sum of the squares of the possible errors, diminishes with the increase of the skill of the marksman, and the mean error as it is generally, although somewhat erroneously, called, is $0.477a$—erroneously, because it is not the mean of the magnitudes of all the errors on the right or left respectively, but is the error the probability of which is the mean of the probabilities of all the errors.

We may vary the statement of the object of our search as follows:—

Suppose the number of marksmen of exactly equal skill to be infinitely great and supernaturally gifted numerators, to count all the shot marks in each of the sections, can we determine *a priori* in terms of the distances of those sections from O, what the number in the corresponding sections should be—or rather can we determine antecedently what the ratio of these numbers in any two sections should be?

The question may also be stated thus—

Suppose that after the shots have been fired at the linear target AB, the bull's-eye were obliterated and only the shot marks remained and a person were called upon to conjecture, from these marks alone, the original position of the bull's-eye, upon what principle would he reasonably proceed?

If the number of marks were very large—practically infinite—there is no doubt that he would correctly take the average of all the distances from any fixed point A in the target—*i.e.*, he would take the sum of the distances from A and divide by the number of shot marks. But it is by no means so obvious when the number is finite—indeed, unless he was assured that all the marksmen were of equal, or very nearly equal skill, it seems almost certain that he ought not to take

that average. Still, remembering that he has no *a priori* hint of any such differences of skill, the temptation to assume the average is very great, and when all the shots come from the same marksman under identical circumstances, the temptation to regard the law of the average, or arithmetic mean, as axiomatic becomes absolutely irresistible.

But if this law of the average or arithmetic mean be conceded as axiomatic, then the law of facility of individual errors results demonstrably, and is given by the formula :—

$$\mathrm{A}e^{\frac{-x^2}{a^2}}dx$$

where

$$\mathrm{A}\int_{-\infty}^{-\infty} e^{\frac{-x^2}{a^2}}dx = 1 \text{ or } \mathrm{A} = \frac{1}{a\sqrt{\pi}}$$

i.e., in the linear target case, if all the marksmen were of absolutely equal skill, then the number of the shots in the sections m inches and n inches respectively from O would be to each other, the total number of shots being very great, as

$$e^{\frac{-m^2}{a^2}} : e^{\frac{-n^2}{a^2}}$$

where a is some quantity dependent upon the skill of the marksmen—the smaller the greater that skill—in fact a^2 is the sum of the squares of all the distances from O.

You will observe, then, to what point we have already arrived on the assumption of the arithmetic mean, for the most probable value of the position of the bull's-eye on the linear target with marksmen of absolutely equal skill, viz., the number of

shots within any small section distant x from the bull's-eye, of length dx

$$= \mathrm{A}e^{\frac{-x^2}{a^2}} dx$$

and this gives conversely the probable distance of the bull's-eye derived from n shots distances $x_1, x_2, \ldots x_n$ from A.

$$= \frac{x_1 + x_2 + \ldots x_n}{n}.$$

The three following propositions have in fact been established by mathematical demonstration.

(1).—If the number of marks is infinitely great, the A.M. certainly gives the most probable result.

(2).—If they are finite, the A.M. gives the most probable result, when all the marksmen are of equal skill and if the law of facility be—

$$\mathrm{A}\epsilon^{\frac{-x^2}{a^2}} dx.$$

(3).—When they are finite, *if* the arithmetic mean certainly gives the most probable position and *if* there be any law of facility, then

(*a*.)—All the marksmen must have been of equal skill.

(*b*.)—The law of facility must have been—

$$\mathrm{A}\epsilon^{\frac{-x^2}{a^2}} dx.$$

The demonstrations are given at the end of the paper.

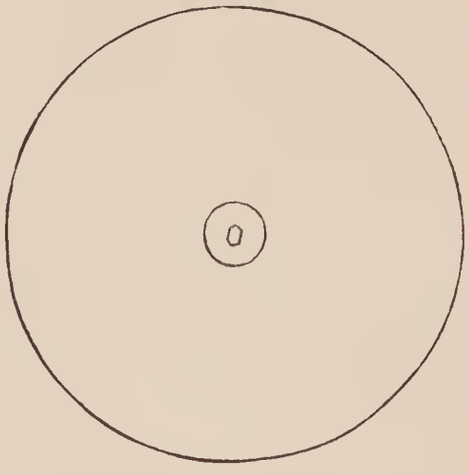
If the marksmen were *not* of equal skill, but if the quantity a in the formula $Ae^{\frac{-x^2}{a^2}}$ were for the different marksmen replaced by $a_1, a_2, \ldots a_n$, then it would be easy to show that the arithmetic mean must be replaced by

$$\frac{\frac{x_1}{a_1^2} + \frac{x_2}{a_2^2} + \cdots \frac{x_n}{a_n^2}}{\frac{1}{a_1^2} + \frac{1}{a_2^2} + \cdots \frac{1}{a_n^2}}$$

or the different distances x_1, x_2, &c., must be weighted, or multiplied by $\frac{1}{a_1^2}$, $\frac{1}{a_2^2}$, &c., quantities which measure the respective skills.

I do not stop to prove this because it is not doubtful, and I do not wish to weary those who are not familiar with symbolical treatment.

FIG. 2.



If the linear target were replaced by a plane target, with the bull's-eye at O, then the chance of any shot mark lying between two circles of radii r and $r + dr$, from O as centre, may easily be found on the above principles to be

$$A\epsilon^{\frac{-r^2}{a^2}} r dr$$

An interesting question arises here: if we knew beforehand the skill of the different marksmen on the linear target we know (as shown above) how to weight the different shot marks. In the absence of that knowledge we assume a general average skill in each, and so arrive by a rough approximation at the arithmetic mean. Can we frame any law according to which, with the *a priori ignorance* of these differences of skill, we may from the observations themselves learn how to weight them?

The most appropriate treatment would appear to be as a first approximation, assume the skill to equal throughout, and determine O by the law of arithmetic mean. Then weight the different shot marks according to their proximity to O, and so by a second approximation determine O and so forth.

It is beside the purpose of this paper to go into further detail in this part of the subject.

It is, of course, almost needless to mention that the linear target shot marks are merely an illustrative instance of a large class of phenomena which admit of similar treatment.

For example, an observer taking readings of an instrument or noting the time of transit of a star, or the elevation of a star, or the angular distance of a star from the moon, or from another star—or a person endeavouring to pace exactly one foot, or to measure accurately one inch, and the like, have the same basis of principle as the linear target and admit of similar treatment.

And the great question we are putting is whether there is any law of facility of error common to all these cases.

To this question we can at present give, as I have said, no absolutely complete and demonstrable answer; but we can say, I think, that very many things conspire to point to the result above stated, which is very generally accepted and acted on by observers and the like, viz.:—That there is a law of facility of error, the same *in form* in all possible cases, but differing in

what mathematicians call the constants entering into that form from case to case, and that this law is—

$$\frac{1}{a\sqrt{\pi}} \; e^{-\frac{(h-x)^2}{a^2}} \; dx$$

The expression meaning that the chance of any error lying between x and $x + dx$, where dx is very small, is given by the above formula where h and a are small quantities differing with different cases, and perhaps in most cases h is zero.

Now, as I have said, to make use of this law as an actual historical and objective law governing phenomena is very precarious; nevertheless with due precautions this may, I think, be done with fairly reliable results.

Mr. F. Galton, whose books on heredity and natural inheritance will most likely be familiar to many of you, has a singular confidence in this law of error, and takes it for his guide in much of his reasoning with an unqualified and absolute reliance, which must sometimes strike us as almost perilous in his abandonment to it; and it is on a question from him on some of these deductions that I wish to speak to you this evening and elicit comments on my mode of dealing with it, but, before doing so, it would be well to shew you some of his illustrative results.

To do so let me return to the linear target, omitting, as I have said, the quantity h, and taking the error law to be

$$\frac{1}{a\sqrt{\pi}} \epsilon^{-\frac{x_2}{a^2}} \; dx$$

inasmuch as no principle is involved in such a simplification.

Well, then, suppose an infinite number of marksmen of absolutely equal skill to be aiming at O, Fig. 1.

The chance of each hitting the target in the sections PP′, P_1P_1' at the same distance m inches to the right and left respectively of O will be the same, and each $=\epsilon^{\frac{-m^2}{a^2}}$

For the sections QQ′ and Q_1Q_1' they will be equal, and each

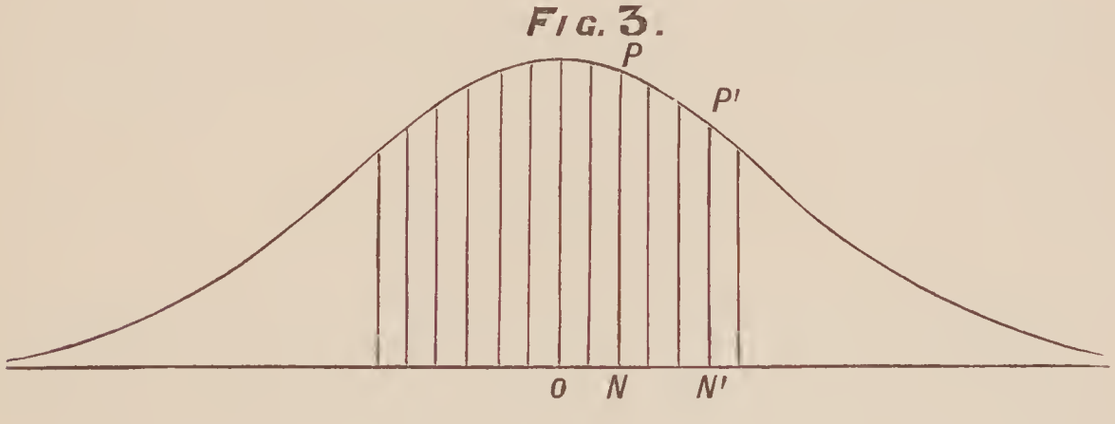
$$=\epsilon^{\frac{-n^2}{a^2}}$$

less than the former, because n is greater than m, and therefore

$$\epsilon^{\frac{-n^2}{a^2}} \text{ less than } \epsilon^{\frac{-m^2}{a^2}}.$$

Now the chances for each marksman being identical for the same sections, because a is the same throughout, it follows, of course, that in any very large number of shots the number of marks in the sections will be proportional to these chances. Let us now

Fig. 3.

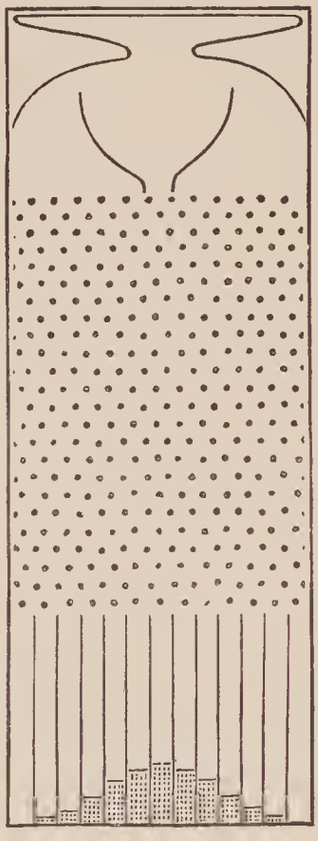


at different distances to the right and left of O draw lines perpendicular to the target proportional to the number of marks in the adjacent sections, and join their extremities (see Fig. 3). We shall obviously have a curve as in the figure, sloping off to the right and left, never quite touching the target line, but doing so sensibly at no great distance from O, and having that line for an asymptote. If the number of shot marks were very great indeed, the lengths of the uprights would at length hardly differ at all from the aforesaid values; and from this it follows that, with supernaturally gifted enumerators, we might, by

counting the number of shot marks in any two of the given sections, actually obtain the numerical value of (a), supposing that to be unknown.

Mr. Galton has devised an ingenious contrivance for making these results sensible.

FIG. 4.



Take a frame glazed in front, having a depth of about a quarter of an inch behind the glass. Place strips in the upper part to act as a funnel. Below the outlet of the funnel let there be a succession of many horizontal rows of pins stuck squarely into the back board and below these again a series of vertical compartments. In this frame let a great number of small shot be enclosed. When the frame is held topsy-turvy all the shot run to the upper end, then when it is turned back again with the funnel uppermost the desired action commences. Lateral strips shown in the diagram have the effect of directing all the shot into the wide mouth of the funnel. The shot passes through the funnel, and, issuing from its upper end, scampers deviously down through the pins in a curious and interesting way, each of them

darting a step to the right or left, as the case may be, every time it strikes a pin.

The pins are disposed in a quincunx fashion, so that descending shots strike against pins in successive rows. The bottom compartment vertically under the mouth of the funnel represents the bull's-eye in our linear target, and if there were no deviation-producing pins all the shot would descend into that compartment, but by reason of these deviations the cascade from the funnel broadens as it descends, and at length all the shot are distributed among the different compartments at the bottom. The greater the number of shot and of the interposed rows of pins the more nearly does the outline of the columns of shot thus accumulated approximate in form to the curve of facility of error as above explained, and it will be found on repetition that the same curve is closely reproduced.

I will call such a contrivance as this a *quincunx*. The general shape of the curve passing through the summits of the shot in the bottom partitions is the same for all quincunxes, that is, it is always

$$y = \frac{1}{\sqrt{\pi}\, a} e^{-\frac{x^2}{a^2}}$$

where x is the distance of any partition from the middle point O, and y the height of the shot in that partition ; but the constant (a) in each quincunx varies from quincunx to quincunx, depending upon the number and size of the shot, rows of pins, and breadth of partitions, the shape of the funnel and the like ; and the laws I have been describing as to error amount to this, that for every conceivable case depending upon deviations from a certain mark or object there is a special quincunx. Every individual has an infinite number of quincunxes. He has his own special quincunx for shooting at a mark, for taking an observation, for making a particular stroke at billiards. and the like.

There are yet further laws of error to be described.

If in the linear target (Fig. 1) any given person aims at O, the chance of his shot lying in PP′, is as we know

$$\frac{1}{a\sqrt{\pi}} e^{\frac{-x^2}{a^2}} dx$$

where (a) is his quincunx constant for shooting.

Now, suppose a second person, whose quincunx constant for shooting is (b), to aim at one of A's shot marks (of course he aims at random at *any* one), what will be the chance of his shot falling at a section x from O ?

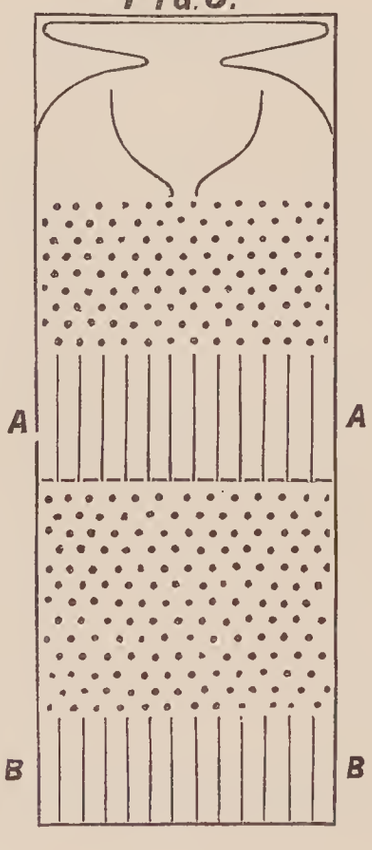
The answer is still of the same form as before, viz :—

$$\frac{1}{\sqrt{\pi a}} e^{\frac{-x^2}{h^2}} dx \quad \text{where } h = \sqrt{a^2+b^2}$$

that is, the quincunx arrangement still holds good, and the compound quincunx constant is the square root of the sum of the squares of the constants of the simple quincunxes.

This result Mr. Galton has also illustrated as follows—

FIG. 5.



Take a quincunx, and any distance down place a number of vertical partitions B, closed at the bottom by a slide.

Let the shot fall through the funnel and be caught in the B partitions, the curve passing through the tops of the columns in each partition will be, as drawn, viz., the facility curve with a certain constant (a).

Below the B's let there be another series of pins, and at the bottom a set of vertical partitions A. Then if the slide be suddenly withdrawn from below the B's the shot will again descend into the A's and will there be arranged in a second curve with some new constant h

If the slide had been withdrawn from only *one* of the B partitions, then it would be found that the curve outline of the shot in the A partitions would have been of the $e^{\frac{-x^2}{b^2}}$ form the origin being at the centre of that one of the A partitions vertically below the B partition so emptied, and with a certain constant (b) and if the slide were withdrawn from all the B partitions, and these had been accordingly discharged the outline curve in the A partitions would be of the $e^{\frac{-x^2}{h^2}}$ form the origin being at the middle A partition, and the error constant h such that

$$h^2 = a^2 + b^2,$$

Or, in other words, if you had an observation of a quantity, subject to the $e^{\frac{-x^2}{a_2}}$ law, and another of a second quantity subject to the $e^{\frac{-x^2}{b^2}}$ law, then the error of a quantity found by adding together the two observations would be subject to the $e^{\frac{-x^2}{a^2+b^2}}$ law.

For example, a man tries to cut off from a piece of tape a portion of one inch with error law $e^{\frac{-x^2}{a^2}}$ and a second man tries to cut off a piece of one inch with error law $e^{\frac{-x^2}{b^2}}$ Then the error law of deviation by x from two inches in the two pieces joined together would be

$$e^{\frac{-x^2}{a^2+b^2}}$$

And observe also, for it is *important*, that exactly the same law holds of the difference of observations as of their sum.

A man tries to cut off a piece of tape of 2 inches, error law $e^{\frac{-x^2}{a^2}}$ and a second man tries to cut off a piece of 1 inch, his error law being $e^{\frac{-x^2}{b^2}}$ then the chance that the difference of the two pieces shall differ from one inch by x is

$$e^{\frac{-x^2}{a^2+b^2}}$$

as before.

In this particular illustration of the attempt to cut off pieces of tape of given lengths, say one inch, there is an apparent difficulty in the application of the error law. We assume the chances of deviation from one inch by x to $x+dx$ to be $Ae^{\frac{-x^2}{a^2}}dx$ the same in defect and excess; whereas it is obvious that when x is nearly equal to one inch and is in defect, the obvious smallness of the part cut off must exclude such a case from an ordinary error, and where x is greater than one inch, the error becomes absolutely impossible. The difficulty, however, is only apparent. We are assuming that all errors are very small, so small that (a) (which measures the mean error) is a very small fraction indeed of the length aimed at, viz., one inch—so that for all values of x at all approaching one inch, the fraction

$\frac{x}{a}$ and *a fortiori* $\frac{x^2}{a^2}$ is extremely large and the quantity $e^{-\frac{x^2}{a^2}}$ absolutely inappreciable. Therefore, while we are not allowed to exclude the *possibility* of such cases, their probability is so minute that their number may be excluded without practical effect.

And now, if I have not wearied you, I come to one main object of this paper.

I have said we must be careful, in applying these principles, to remember always that we are dealing statistically—we are not treating each observation or effort historically so as to trace its history throughout, in which case we might of course use positive language, and say definitely such and such will be the case, but we are speaking of things in which ignorance is the very basis of our knowledge, chaos the groundwork of our order. If we are tossing up a coin—not weighted—100 times in succession, the statistical expectation is 50 heads and 50 tails. But if on the first two tosses a head comes up, there is no law to be invoked, the *expectation* of the remainder is not affected, but is 49 heads and 49 tails. You all know that popular opinion is too firmly rooted to be capable of education on this point. You will see the players at any purely gambling game like, *rouge et noir*, intently watching the record of the runs before putting down their stake. I do not doubt you remember how a year or so ago, a man thought it worth while to send a letter to the *Times*, from some distant land, affirming, with corroborative evidence, that he saw a deal at whist in which the dealer held all the trumps. It was as noteworthy as if you or I had written to say that on taking up our hand we held the ace, three and seven of clubs, king, queen, ten, four and three of hearts, knave, five and eight of diamonds, and ten and six of spades. However, as I say, we must beware of the kind of expectations we base upon, and the confidence we repose in, these probable conclusions.

Now Mr. F. Galton, in his works on hereditary genius, natural inheritance, and the like, is necessarily compelled to base

his inferences very generally upon these statistical inferences—so much so, that to some minds he may appear to run into the danger I have pointed out and to draw conclusions from them as if from objective natural laws. His enthusiasm for them is unbounded. In his "Natural Inheritance" he says—"I know of "scarcely anything so apt to impress the imagination as the "wonderful form of cosmic order expressed by the 'law of frequency "in error.' The law would have been personified by the Greeks "and deified if they had known of it. It reigns with serenity and "with complete self-effacement amidst the wildest confusion. "The huger the mob and the greater the apparent anarchy, the "more perfect is its sway. It is the supreme law of unreason. "Whenever a large sample of chaotic elements are taken in hand "and marshalled in the order of their magnitude, an unsuspected "and most beautiful form of regularity proves to have been latent "all along. The tops of the marshalled row form a flowing curve "of invariable proportions, and each element as it is sorted "into place finds as it were a preordained niche accurately adapted "to fit it. If the measurement at any two specified grades in the "row are known, those that will be found at every other grade, "except towards the extreme ends, can be predicted in the way "already explained and with much precision."

In Fig. 3, let

$$\mathrm{ON} = x \quad \mathrm{NP} = \mathrm{A}e^{-\frac{x^2}{a^2}}$$

$$\mathrm{ON}' = y \quad \mathrm{N'P'} = \mathrm{A}e^{-\frac{y^2}{b^2}}$$

$$\therefore \quad \frac{\mathrm{NP}}{\mathrm{N'P'}} = \frac{e^{-\frac{x^2}{a^2}}}{e^{-\frac{y^2}{b^2}}} = e^{-\frac{x^2-y^2}{a^2}}$$

or,

$$\frac{a^2}{x^2-y^2} = \log\frac{\mathrm{NP}}{\mathrm{N'P'}}$$

$$\therefore \quad a^2 = (x^2-y^2)\log\frac{\mathrm{NP}}{\mathrm{N'P'}}$$

i.e., if the heights of the shot in any two departments of the quincunx, distant each x and y from the middle compartment respectively be known, then a^2 or a is known, and the height in any other compartment is also known.

Depending, then, with this absolute reliance on these statistical conclusions, Mr. Galton is in the habit, as he says, confirmed by constant and consistent measurements, of regarding different groups of phenomena presenting themselves in certain physiological investigations as being as absolutely subject to these laws of facility as if they were objectively impressed upon them. He regards the error, or what I have called the quincunx constants, in these groups as properties inherent in the groups, which may for each group be determined by measurement, and, thus determined, form the basis of future measurement. I have endeavoured to describe the precautions and limitations under which this treatment should be pursued. Subject to a careful attention to these, many of the inferences may, I think, be accepted. In the course of his investigations into those physiological and biological laws bearing upon heredity, which he has made his peculiar study, Mr. F. Galton encountered a difficulty connected with this law of error, and referred it to a mathematical friend at Cambridge, but they could not come to agreement, and he was unable to make any progress in the development of his theory in consequence of this uncertainty. He referred his difficulty to me, and at first I decidedly took the side against him. It seemed to me that he was falling into that dangerous and tempting misuse of the science of probabilities of which I have already spoken. But gradually, and upon further reflection, I began to feel that this view of his treatment was not quite correct; and at length I found myself, at least with due precautions in the enunciation, coming over to his side. Other mathematicians had been appealed to, with a curious variety of result; so that, notwithstanding the great apparent simplicity of the issue, the matter remains undecided, and I rather hoped to elicit a discussion this evening. In this hope I fear I shall be disappointed, as our professedly mathematical members are not

present at our meeting. Before giving the abstract question submitted by Mr. Galton, it will be interesting to state the general nature of the enquiries which led up to it in Mr. Galton's own words. He says:—

"When two sets of occurrences are said to be correlated, the meaning that lies behind the phrase is that their variations are governed partly by causes common to both, and partly by causes special to each. I want to analyse and to separate these three sets of causes, and to learn their relative importance. Now I had already got a long way in that direction during my enquiries in heredity, which is a special instance of the general subject of correlation, persons who are kinsmen resembling each other, on the whole, more nearly than those who are not, on account of a certain dose of common biological conditions. It turned out, as I have often explained, to be both an observed fact and a theoretical necessity, that a son taken at random shall, on the average, not resemble his parents, but be more mediocre than they. That a man taken at random shall, on the average, not resemble a particular brother, but be more mediocre than he. There always exists what I called a 'regression' in family likeness. I also showed that all relationship and all correlation is expressible by a simple formula whenever each member of the pairs of associated variables is itself a normal variable. Let us call any two classes of associated variables by the letters A and B, and let the mean of all the A values be called $\overline{a}$, and that of all the B values $\overline{b}$. By hypothesis the individual A values are distributed normally round $\overline{a}$, and the B values normally round $\overline{b}$. Let A_x be some particular value of A, which we will write in the form $\overline{a}+x$. Let B_{y1}, B_{y2}, etc., be the various B values that have been observed to occur in connection with A_x. Write these in the form of $\overline{b}+y_1$, $\overline{b}+y_2$, etc., and call the mean of all of them $\overline{b}+\overline{y}$. Then the law is that x is equal to $\overline{y}$, multiplied by a certain quantity r, which is less than one, and which is the same for all values of x; that is $x=r\overline{y}$. This constant r expresses what I have called the ratio of regression. The reason why r

is and must be constant for all values of x is explained in my book, 'Natural Inheritance.' Now, I want to go a step further and to use this r as a means of determining the variability of that group of influences when taken by themselves, that are common to any pairs of correlated values, and consequently of determining the variability of the groups of influences that are special to each member of the pair; and it was when making this step that a certain problem occurred." In its simplest form the problem is stated by Mr. Galton as follows:—

Suppose a man to attempt to cover one foot by a hop, and then the same or another man to attempt, starting from the end of the hop, to cover another foot by a stride, and suppose you *know* that the law of error constant of the hopper was (a) and that of the strider was (b), you know by what I have already said that the law of error of the resultant of the hop and stride is $e^{\frac{x^2}{h^2}}$ where $h^2=a^2+b^2$. Suppose then the given elements to be modified, and you do not know the law of error constant of the strider (b), but you do know that of the *resultant*. of the hop and stride (h) as well as that of the hopper, may you not infer that the error constant of the strider is b where $b^2=h^2-a^2$.

In fact, may you not treat the formula $h^2=a^2+b^2$ as an ordinary equation, and deduce from it $b^2=h^2-a^2$.

My first impulse was to say, *decidedly No*, you must infer that the skill constant of the strider was $\sqrt{h^2+a^2}$ and not $\sqrt{h^2-a^2}$.

I said it is really a case of composite error, in which the error constant is, as stated above, the square root of the sum of the squares of the component error constants, equally in the case of a difference and a sum.

Here, I said, you have a series of observations or results, viz., the sum of stride and hop, whose variations by defect or excess from a certain mean result follow the law of error with error constant (h), and you have also a series of hops deviating,

according to the error law, with constant (a), then by our general principle the remainder or stride error, the stride being the *difference* of the total and the hop, must follow the same law with constant $\sqrt{h^2+a^2}$.

As already stated, I understood from Mr. Galton, he had been previously in communication with another mathematician who had arrived at this conclusion (and for all I know still adheres to it), and it was in his dissatisfaction with this conclusion, and his desire to elicit one more in accordance with his own treatment, that he had appealed to me. And this is not to be wondered at when we remember the very positive and objective interpretation which, as I have said, Mr. Galton gives to the meaning of this law in the applications he makes of it. I thought that the apparent discrepancy must be referable to this too, perhaps, illegitimate use of the law, much, for instance, as if, to recur to a previous illustration, a man, knowing that a fairly weighted shilling ought by the law of chances to come up 50 heads and 50 tails in 100 tosses, were to conclude after two tosses with heads that the expectation of the remaining 98 tosses will be 50 tails and 48 heads. Therefore I could do no other than conclude, with my predecessor, that I ought to stick to the $\sqrt{h^2+a^2}$ and not give in to the $\sqrt{h^2-a^2}$. And yet I had misgivings. I could not help feeling that by a practically indefinite increase of the number of cases or trials the results, based on probabilities, ought to become practical certainties, and, therefore, to be completely accordant; whereas the $\sqrt{h^2+a^2}$ result left an absolutely undiminishable discrepancy between the arrangement of the errors of the striders as leading to that of the composites, and the same as inferred from the known errors in these composites. But yet it was difficult to see wherein the difference of treatment in the two cases consisted. To do this I wanted to ascertain upon what conceivable hypothesis we might accept the Galton view of the a, b, and h as determinate—almost physical—properties of our respective groups. For although it may be said that both the

discrepant results accept the legitimacy of regarding a, b, and h as ascertainable and definite, and that the question is not how do we get them, but what use do we make of them when we get them, yet I cannot help thinking that our confidence in this case may be modified by our knowledge of the means by which they are ascertained, and for convenience sake I will just a little vary the practical details. I will take the first question to be: Let the *same* man be employed for a certain number of hours on consecutive days, under the same physical conditions, to cut off pieces of tape to the best of his judgment of one inch in length each, and when the total so cut off has reached a very large number, let the number so cut off be put in a box (A), and let any man be employed in sorting out those differing from one inch in excess or defect by the same lengths, say between ·01 and ·01001 of an inch, ·02 and ·02001, and so on. Then may we not infer, with almost *practical* certainty that the number of tapes in the box with differences from one inch lying between x and $x + \frac{1}{10000}$ would be

$$\mathrm{A}e^{-\frac{x^2}{a^2}} \times \tfrac{1}{10000}$$

where $\mathrm{A}\int_{-\infty}^{\infty} e^{-\frac{x^2}{a^2}}\,dx = m$, the total number of pieces cut off.

So that if we could get numerators to count up all the pieces between these limits and those between two others as y and $y + \frac{1}{10000}$ we might, as above shown, determine the numbers within any other two limits. In fact we should establish the existence of a real error constant for the tapes cut off by the man and put into A. Just so let another man cut off tapes, each as nearly equal to one inch as possible, and let them when their number is very large be cast into a second box (B). We may, as before, determine a practical meaning of the error constant b of the tapes in this second box, and might determine that for all practical purposes, and very approximately when n

is very large the number of the B bits of tape between y and $y+\frac{1}{10000}$ would be $Be^{-\frac{y^2}{b^2}} \times \frac{1}{10000}$.

Now then you have two boxes composed of m and n tapes of these varying lengths, such that the more you increase m and n, the nearer do these numbers represent the *actual* numbers of tapes of those lengths, and when m and n are very large indeed, they may be regarded as completely so representing them.

From this point it is a mere matter of computation of combinations to say that if you took out a tape at random from each of the m tapes in A, and one from each of the n tapes in B, and put into a third box C a piece of tape accurately equal to the sum of the lengths of the two tapes so extracted, there will be in all $m\,n$ tapes in C, and the number of these differing from one inch by between z and $z+\frac{1}{10000}$ would be $Ce^{\frac{-z^2}{a^2+b^2}} \times \frac{1}{10000}$, this being true to a degree of accuracy increasing as before with m and n, and commensurable with the accuracy of the inferred principle of assortment in the A and B boxes.

If, therefore, with the aid of laborious computators we evaluated these numbers for two values of z we obtain the value of h^2, and this *must* be equal to a^2+b^2, so that if you did not previously know either a or b you now know a^2+b^2 *quam proxime*. I do not see how any one could object to this conclusion, so that if we also knew *(a)* we should get

$$b^2=h^2-a^2,$$

or the Galtonian contention is verified.

What, then, is the meaning of the result, which certainly looked most plausible, giving $b^2=h^2+a^2$, and which, so far as I know, is even now maintained to be the correct answer to our question? To me, it seems that the problem leading to this result is equivalent to this.

You take the $(m\ n)$ tapes in the C box, the number between z and $z+\frac{1}{10000}$ being

$$Ce^{\frac{-z^2}{h^2}} \times \frac{1}{10000}$$

as above, where h is supposed to have a determinate meaning, and to be capable of estimation, as above explained.

And similarly you take the m tapes in the A box, the number between x and $x+\frac{1}{10000}$, being $Ae^{\frac{-x^2}{a^2}} \times \frac{1}{10000}$, *or* a measurable and measured quantity.

You combine those two and two as before, and put into a third box (D) m^2n tapes, the length of each being equal to the *difference* in lengths of the tapes in each combination. Then I say that, as a matter of mere combinations, the number out of these m^2n, whose difference from one inch is between y and $y+\frac{1}{10000}$, will be

$$De^{\frac{-y^2}{h^2+a^2}} \times \frac{1}{10000}$$

This is a true conclusion, but is not, I think, Galton's question.

We may see this the more clearly if we divest both problems, as we may do, of all language of possibility or uncertainty,

(1) I have a collection of a given number of tapes of known distribution $e^{\frac{-x^2}{h^2}}dx$ law, *i.e.*, the number of tapes of different lengths being given, and I know that this collection *has been formed* by taking two and two at random, one from these and one from another given number of known distribution $\left(e^{\frac{-x^2}{b^2}}dx \text{ law}\right)$, and another from a third number of unknown distribution $\left((e^{\frac{-x^2}{b^2}}dx) \text{ law, where } b \text{ is not known}\right)$, then I know that h^2 must be a^2+b^2 and therefore $b^2=h^2-a^2$.

(2) I have a collection of a given number of tapes of known distribution $\left(e^{\frac{-x^2}{h^2}} dx \right)$, and with these I combine at random two and two, one from each, a collection of another given number of tapes of known distribution $\left(e^{\frac{-x^2}{b^2}} dx \right)$, then there is equally no uncertainty about the resulting distribution, and that it must be $e^{\frac{-x^2}{a^2 + h_2}} dx$ law, but this is a different case from the last.

It is stated in the foregoing that if it may be assumed that the law of error of any finite number of marksmen at a linear target is for each $Ae^{\frac{-x^2}{a^2}}$ *i.e.*, the same in form and error constant for each; then the most probable position of the bull's-eye, as inferred from the shot marks alone, is the arithmetic mean.

For let any point in the linear target be taken as origin, and let the distances of the marks from it be $x_1, x_2, \ldots x_n$, and let $\bar{x}$ be the unknown distance of the bull's-eye.

The chance of one of the n shots being at x_1, $x_1 + dx_1$ is

$$Ae^{\frac{-(x_1-\bar{x})^2}{a^2}} dx$$

a second $x_2, x_2 + dx_2$ is $Ae^{\frac{-(x_2-\bar{x}^2)}{a^2}} dx$

and so on.

therefore the chance of the n shots being as supposed is

$$A^n e^{-\frac{\{(x_1-\bar{x})^2+(x_2-\bar{x})^2+\&c.+(x_n-\bar{x})^2\}}{a^2}}$$

and this is greatest when $\bar{x} = \frac{x_1+x_2+\&c.+x_n}{n}$

i.e., the most probable value of the unknown position of the bull's-eye is that given by the arithmetical mean.

If the law of facility of error for the n marksmen had been the same, but the error constants different, as $a_1, a_2, \ldots a_n$, then the quantity to be made a maximum would have been of the form

$$A_1 A_2 \ldots A_n e^{-\left\{\frac{(x_1-\bar{x})^2}{a_1^2}+\frac{(x_2-\bar{x})^2}{a_2^2}+\&c.+\frac{(x_n-\bar{x})^2}{a_n^2}\right\}}$$

giving $$\bar{x}=\frac{\frac{x_1}{a_1^2}+\frac{x_2}{a_2^2}+\&c.+\frac{x_n}{a_n^2}}{\frac{1}{a_1^2}+\frac{1}{a_2^2}+\&c.+\frac{1}{a_n^2}}$$

proving the results above stated.

If, therefore, we have any assurance beforehand, not only of the general form of an error law, but that the marksmen are of equal skill, we are justified in taking the arithmetic mean as the most probable result.

Also, if we have assurance of the form of law and the relative skill of the marksmen, then we may equally obtain the most probable result by duly weighting the marks according to our knowledge of these respective skills.

If we have only the marks to go by, and no antecedent knowledge of the respective skills, even though the general form of error law be admitted, the only course open is to proceed by some method of approximation.

As a first approximation assume equal skill throughout, and thus determine $\bar{x}$ by the arithmetic mean.

For a second approximation weight the given marks according to their nearness to this mean, and then apply the second method, and so on.

Again in the converse problem, as stated above, it may be proved that if the arithmetic mean gives the most probable position, the law, if any law exists, must have been of the $e^{-\frac{x^2}{a^2}}$ form with the error constant the same for each marksman. For suppose x_1, x_2, and x_n to be distances of the

marks from any point in the linear target, and $(\bar{x})$ to be the distance of the mark that was aimed at, then, if there be any general law of facility as $\phi\,(x)$, the *a priori* probability of these errors is proportional to the product $\phi\,(x_1-\bar{x})\,\phi\,(x_2-\bar{x})$ &c. And, therefore, after the marks have been made, the probability that $\bar{x}$ was the true value is also proportional to this expression, and this must, therefore, be a maximum for the true value of $\bar{x}$, *i.e.*,

$$\frac{\phi'(x_1-\bar{x})}{\phi\,(x_1-\bar{x})}+\frac{\phi'(x_2-\bar{x})}{\phi\,(x_2-\bar{x})}+\ldots+\frac{\phi'(x_n-\bar{x})}{\phi\,(x_n-\bar{x})}=0$$

Let $\psi\,(x)=\dfrac{\phi'\,(x)}{\phi\,\,(x)}$ and let $x_2=x_3=\ldots=x_n=x_1-n\alpha$

$$\therefore\ \bar{x}=\frac{x_1+x_2\ldots+x_n}{n}=x_1-(n-1)\alpha$$

$$\therefore\ \frac{\psi\,(n-1\alpha)}{n-1\alpha}=\frac{\psi(-\alpha)}{-\alpha}\text{ for all values of }\alpha$$

$$\therefore\ \frac{\psi(x)}{x}=m\ \text{(some constant)}.$$

But m must be negative, let it be $-\dfrac{2}{a^2}$

$$\therefore\ \phi\,(x)=Ae^{\frac{-x^2}{a^2}}$$

The preceding demonstrations are familiar to mathematicians, and prove the statements above made.

Beyond this point I think we have no absolutely conclusive demonstration of a *general* law of facility of error, but there are many considerations strongly suggestive of the $e^{\frac{-x^2}{a^2}}$ law, when taken under due precautions. As I have already shown, the quantity a^2, if this law did exist for any set of trials or observations, measures the sum of the squares of the possible errors under that law, and (a) measures the probable error as above defined, *i.e.*, not the mean error, but the

error whose chance of occurrence is the mean of all the chances.

My own belief—which I cannot prove—is that for every particular individual, in every particular kind of effort, there is a law of facility of this form with its own particular (a)—when I say of this form, I mean that I take this form for simplicity's sake as representative of the more general form $e^{-\frac{(h-x)^1}{a^2}}$

Many attempts at demonstration on very simple principles have been offered, especially, *e.g.*, that due, I think, originally to Herschell.

This is the case of the plane target with bull's-eye at O. If we may *assume* that in this case the chances of perpendicular deviations from two axes at right angles to each other (Ox, Oy,) are independent, $\phi(x)$ and $\phi(y)$, it is then easy to prove that $\phi(x)$ must be of the form $e^{-\frac{x^2}{a^2}}$; but, then, this assumption of the independence of $\phi(x)$ and $\phi(y)$ involves the very point to be proved, and does not really advance the matter.

We have already seen that, if the chance of any error being between x, $x + dx$ be $Ae^{-\frac{x^2}{x^2}}dx$, and that of any other which is capable of being added to or subtracted from the former lying between y and $y + dy$ be $Be^{-\frac{y^2}{b^2}}dy$, then the chance of the sum or difference of these errors lying between z and $z + dz$ must be $Ce^{-\frac{z^2}{a^2+b^2}}dz$, and this law of composite errors may, of course, be indefinitely extended.

Assuming, therefore, this law of facility to be universal, we know that if an error ϵ, as, for instance, one of observation, be decomposable into any number of other errors, ϵ_1, ϵ_2, $\cdots$ ϵ_n, then

the chance of ϵ lying between u and $u+du$ will be

$$Me^{\frac{-u^2}{a_1^2+a_2^2+\ldots+a_n^2}}du.$$

And, of course, the number (n) may be extended without limit, only that in this case, in order that the result may have any meaning, each (a) must be infinitely small, or, at any rate, the *average* value of each a^2 cannot be greater than $\frac{1}{n}$

Laplace, and Poisson following and improving upon Laplace, have generalised this result, and have proved that, without *any* assumption of a general law of facility, if any number of errors, ϵ_1, ϵ_2, and e_n follow, each of them, arbitrary and independent laws, yet in the limit when their number is very great the law of facility of their sum will be of the aforesaid form, *i.e.*, the chance of

$\underset{n=\infty}{Lt}\ \Sigma\left(\epsilon_1+\epsilon_2\ldots+\epsilon_n\right)$ lying between u and $u+du$, is

$$\frac{1}{\sqrt{\pi}\sqrt{a_1^2+a_2^2+\ldots+a_n^2}}\ e^{\frac{-u^2}{a_1^2+a_2^2+\ldots a_n^2}}\ du.$$

The *a's* in this case have similar meanings to those in the case of the assumed general law $Ae^{\frac{-x^2}{a^2}}$, and are the sums of the squares of all possible errors, with the respectively assumed laws for the ϵ's, and the assumption for any practical application must be that each is very small, or that the average value of each a^2 is of an order less than $\frac{1}{n}$.

Hence it is inferred that as all errors, say of observation, marksmanship, and the like, however apparently simple, may be, and in fact are, the resultants of errors from an infinitely great number of causes, we may base the evidence of a universal law of facility, and the form of that law upon this general proposition.

That is to say that if we regard any observation or effort as subject to errors from a very large number of independent

causes, themselves liable to error according to any assumed laws of facility, these laws being either the same or different and quite arbitrary, subject only to the restriction as to magnitude already noticed, then the law of facility of the resultant error must assume the $e^{\frac{-x^2}{a^2}}$ form; and, therefore, that as it is reasonable to regard any such observation or effort, however apparently simple, as really decomposable into many component efforts, and, therefore, the error in such observation as really the resultant of an infinite number of component errors, we may accept the $e^{\frac{-x^2}{a^2}}$ law as the only universal law of facility; this conclusion is, doubtless, almost irresistible, but some remarks may be made upon it.

In the first place there can be no assignable limit to the decomposition. The so-called component errors must be regarded as themselves decomposable and therefore of necessity, by the aforesaid reasoning, as following the $e^{\frac{-x^2}{a^2}}$ law.

In fact, it is proved that the theorem above enunciated may be generalised as follows, viz. :—

Let the component errors, ϵ_1, $\epsilon_2, \ldots \epsilon_n$, be multiplied by arbitrary multipliers as μ_1, μ_2, $\ldots$ μ_n, then it is proved that whatever be the respective laws of facility as before, the chance of the expression $\mu_1\epsilon_1 + \mu_2\epsilon_2 \ldots + \mu_n\epsilon_n$, lying between u and $u+du$ is $\frac{1}{k\sqrt{\pi}} e^{\frac{-u^2}{k^2}} du$ where $k^2 = \Sigma(\mu^2 a^2)$.

In other words that the law of facility for any linear function of the component ϵ's is the same as for their sum.

But particular linear functions of these component ϵ's are the ϵ's themselves, so that we get the law of facility for each ϵ separately to be $e^{\frac{-x^2}{a^2}}$.

Without insisting upon this conclusion which may be taken rather as indicating a limitation to the second more general form of the Poisson result, we may at any rate affirm, as a demonstrated proposition, that no *universal* law of facility of error can exist except that of the $e^{\frac{-x^2}{a^2}}$ form, and further that by the assumption of this form of universal law, all our results may be harmonised.